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SOME
REFLECTIONS
ON THE
UNCERTAINTY
OF MANY
ASTRONOMICAL
AND
Geographical Poſitions,

With regard to the
FIGURE and MAGNITUDE of the EARTH,
The finding the
LONGITUDE at SEA by WATCHES,
AND OTHER

ASSERTIONS
OF THE
Moſt eminent *Aſtronomers.*

With ſome Hints towards their REFORMATION and EMENDATION.

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G.R. III
BR. MUS.

THE

PREFACE.

I HAVE lately put ſome thoughts into writing about the figure and magnitude of the earth, with a view of expoſing the weakneſs and inſufficiency of the arguments and proofs of thoſe who will have its figure to be ſpheroidical either flat or long, and that the menſurations of its magnitude, as given in the public authors, are not abſolute certainties and infallible truths, being too much ſtained with hypotheſes and tainted with errors of various kinds.

I have alſo added ſome hints and propoſitions hereunto, tending to ſhew the uncertainty of the truth of the Newtonian phyſical part of aſtronomy, and that the ſeveral ſyſtems of the world, given us by the philoſophers and aſtronomers of the order, paths, motions, magnitudes and figures, &c. of the celeſtial bodies, are all uncertain and merely fictitious, no man knowing which is the trueſt, or indeed whether any one of them be ſo or not.

Herein I have particularly obſerved, that the diſtances of places upon the earth, ſituated on the ſea ſhores eſpecially, of whatever figure and magnitude the earth really is, can certainly be beſt and trueſt determined by the inſpection and compariſon of the journals of the ſeamen, who have ſo often ſailed to and from thoſe places; and that by help of ſuch journals we may be enabled to diſcover the earth's true figure and meaſure, the length of its degrees, to as great, if not greater certainty, than by any other method whatſoever.——That ſea journals contain meaſurements of the diſtances of places at ſea, and eſtimative determinations of their longitudes and latitudes, and courſes, and tho' theſe are often imaginary, yet they are oftener true, or nearly ſo, as would appear if enough of theſe journals were inſpected and compared together. The ſeamen are the beſt judges of their own buſineſs, and muſt be allowed to be the moſt fit to remove any faults and deficiencies in their own art of navigating ſhips in the beſt and ſafeſt manner, and in the ſhorteſt time poſſible, from one place to another.——Even the finding the longitude at ſea has been done by the ſailors themſelves, more than ten thouſand times, as their journals will verify and confirm. Nor do I take the art of navigation to be ſo defective as thoſe who are ignorant of it may imagine, for want of ſome other independent method, (like that of finding the difference of latitude,) than that the ſailors know already, and have ſo long been uſed to, that the longitude of places have been almoſt all ſet down in our globes, maps and charts, by the help of eclipſes of the moon or Jupiters ſatellites, founded upon the uncertain ſuppoſition that the times of the beginnings, middles and endings of ſuch eclipſes, are always proportional to the longitudes of the places.——That the aſtronomers do not agree about the equation of time, and the inequality of the length of the natural days, nor is the truth of the tables of the equation of time, abſolutely to be relied upon, becauſe they are all founded upon, and computed from different and uncertain principles.——*Ptolomy* and many others will have the equation of time to be the difference between the ſun's mean motion in the ecliptick, and his right aſcenſion.—*Kepler* will have the cauſe of this to be the unequal motion of the earth about its axis—*Vendeline* and others will have all the tables of the equation of time to be falſe and uſeleſs, and makes no difference between the true and apparent time. The late Mr. *Huygens*'s table of the equation of the natural days, given us in his famous book of the pendulum clock, widely differs from the late Mr. *Flamſtead*'s *ſtead*'s table. This laſt makes the greateſt difference between the mean and apparent time, to be about a quarter of an hour; and Mr. *Huygens*'s table makes that greateſt difference to be half an hour. Theſe are things which deſtroy the veracity of the tables of the equation of natural days, and makes void their uſe in a great meaſure.----Alſo I do affirm, the longitude at ſea cannot be found out, by any watch or time-piece whatſoever, unleſs that watch be of ſuch a curious and extraordinary conſtruction as to meaſure time equably at all places and ſeaſons of the year; and by means thereof, the true hour of the day can be obtained when the ſun is upon the meridians of all places at ſea, in all latitudes and ſeaſons of the year. And whoever ſays he has made ſuch a watch, is not to be believed till that watch be tried by experience at ſea, at leaſt in one hundred voyages from ſome of our ports to remote places abroad.---All theſe articles, and many others of the like kind in the courſe of this work, are enlarged, and their truth, as I think, are ſufficiently made out, and clear at leaſt to me.

I have no mercenary views in writing this, it is only for the ſake of truth; and an endeavour to hinder the young geographers and ſailors of this age and nation from being miſled in a ſubject which is the chief ſupport and foundation of the art of navigation, and make them more cautious of confiding in and uſing any new revived method for finding the longitude at ſea without good proof. Both aſtronomy and geography, are very much corrupted and mixed with error and uncertainty, and the beſt way to diſcover and remove many of them, are by the ſeamen themſelves and their journals. Aſtronomy will never be freed from error and uncertainty, till experience be more regarded, and made uſe of by multitudes of perſons furniſhed with inſtruments fit for obſervation and meaſurements, ſtationed at proper places of the earth, acting by correſpondence, joint concert, and prior agreement with one another.——The kings and princes of the earth ſhould each have a college of aſtronomers under the management of an able maſter, and other ſubordinate directors, that may learn the ſcholars and youth the rudiment of arithmetick, geometry, and mechanicks, and the moſt uſeful and eaſy parts of practical aſtronomy, and make them fit for ſo much and no more of theſe things as are neceſſary for the uſeful purpoſes of chronology, geography and navigation, and ſend them out to all parts of the earth to make obſervations and meaſurements. It is far better to obtain true aſtronomical concluſions from the repeated experience of many, than truſt to the truth of what has been obtained from a few.——The theory ſhould rather be confirmed and deduced from real practice, than the practice from that. Does not every expert ſailor, by long uſe and practice, know the rate of going of his ſhip, with ſuch a wind and courſe, and ſeldom fails much of his right reckoning and eſtimation of the diſtance between two places, unleſs his voyage be attended with ſtorms and bad weather, and other croſs accidents. Therefore, by means of ſea journals, enough of which by command and deſire may be gotten from the ſea officers and maſters of ſhips, already prepared and fit for uſe, tables of the diſtance of places upon all the points of the compaſs may be ſet down for the future uſe of others who are to ſail to thoſe places, much more to be relied upon for exactneſs, than the diſtances of places obtained from the latitudes and longitudes of the places by trigonometrical computation founded upon theory and hypotheſis, and the uncertain figure and magnitude of the earth.——In a word, if ſuch methods as theſe had been taken and purſued for this one hundred and fifty years laſt paſt, a true, uſeful and eaſy practical aſtronomy, and geography, would have been obtained long before now, and the art of navigation arrived to its greateſt perfection: nor would there have been occaſion for publiſhing rewards of twenty thouſand pounds by authority for the diſcovery of the longitude at ſea, which has hitherto had no other effect than ſetting all heads and hands at work, and producing, for the ſake of intereſt, abundance of quacks and impoſſible ſchemes for its diſcovery. The late Meſſ. *Whiſton* and *Ditton*, were laughed at for what they had wrote about finding the longitude; and in the year 1722 the diſcovery of the longitude, was made one of the bubbles in *Change-Ally*, and put up to be ſold to the beſt bidder. Many brokers, crazy projectors, and poor crafty knaves, cryed it at *Garraway's Coffee-Houſe*, along with beech, oil, *Welch* copper and *South-ſea* ſtock. Ignorance, vanity, ſelf-conceit and intereſt, were all at work at that time, but produced nothing; and there ever have been ſome that take a pride in arduous effections, which much wiſer men than themſelves know cannot be accompliſhed at all.

N. B. I once more repeat it, that ſea journals are accounts of the meaſurements of the diſtance of places, at ſea or near it, and tho' they do not all agree, and give the ſame diſtance between two places, yet the medium diſtance obtain'd from that of many of the journals will be nearly the true diſtance, and by this means the longitudes, latitudes, and courſes may be had near the truth, of all ſea-port towns, from the records of thoſe who have by rational eſtimation, long and repeated uſe, and diligent obſervation, traced out their ſhip's way, and place every day at noon, on the ocean, verified by a good obſervation (if it can be had) of the latitude each day.——The bulk of the ſeamen who navigate ſhips do their buſineſs always with diligence, earneſtneſs, and their utmoſt ſkill, being induced thereto from the greateſt motives imaginable, *viz.* the ſafety of their own lives, ſhips, and fortunes. And for this reaſon I am perſuaded their journals have a good deal of truth in them.——But I cannot ſay the ſame of the operations of many of our aſtronomers, who have always been finding fault with, and charging the ſeamen of error in their buſineſs of the art of navigation, eſpecially with regard to the adjuſtment of the log line and half minute glaſs, when it is they themſelves who really ought to be blamed, and not the ſeamen, for ſpending too much time in trifling, and almoſt uſeleſs obſervations, to make theſe agree with and confirm the truth of ſome imaginary ſyſtem of the world, or favorite theory or other, that has no real exiſtence at all. A very little aſtronomy is ſufficient for a ſailor, who does not want to be perplexed, and interrupted in his buſineſs by the productions even of the moſt able aſtronomers of theſe latter ages; nor do I think it in the power of theſe laſt to afford any great aſſiſtance to the ſailors in their own art.——I had almoſt forgot to obſerve, that the finding the longitude at ſea, by means of a clock or time-piece, has been attempted many years ago.——The great mathematician Mr. *Huygens* contrived and got clocks prepared for this purpoſe above one hundred years ago, which were carried to ſea to try how they would perform in ſeveral voyages, as may be ſeen more at large at page 16, 17, and 18, of his famous book of the Pendulum Clock, publiſhed at *Paris* in the year 1657. But the events did not turn out according to his wiſh, nor did not ſo happily ſucceed, as to eſtabliſh their future uſe and certainty in finding the longitude at ſea. Almoſt all the great mathematicians and able mechanicks of this nation, who have been fellows of the royal ſociety, but are now dead, deſpaired of finding the longitude by a clock or watch; and I myſelf well remember to have heard the late Mr. *George Graham*, (the moſt able clock, watch, and aſtronomical inſtrument maker in the world, it may be) more than once ſay, he doubted whether or no the longitude could ever be found out by a clock or watch. This was his anſwer to thoſe who aſked his opinion about it.——He was well acquainted with the difficulties lying in the way, and never thought any of his own inſtruments would do, whether clock or watch.—If Mr. *Graham* was of this opinion, it is reaſonable to ſuppoſe his conſtant friend and companion the late Dr. *Halley*, the royal aſtronomer at *Greenwich*,

wich, thought ſo too.——Alſo *Varenius*, in his geography, ſee Prop. 7. chap. 31. and Dr. *Jurin*, did not think it ſafe to truſt to a clock in finding the longitude at ſea, at leaſt till ſome of the difficulties he mentions were removed by experiments.--If Mr. *Graham* afterwards altered his opinion, upon what he ſaw and knew of Mr. *Harriſon*'s clock or watch, I cannot preſume to ſay. In ſhort, it is only a great multiplicity of experiments with theſe watches of Mr. *Harriſon*'s, that can prove their utility, and nothing elſe in the diſcovery of the longitude at ſea.

SOME
OBSERVATIONS
ON THE
Figure and Magnitude of the EARTH,

With a view of expoſing the weakneſs and inſufficiency of the arguments and proofs of thoſe who will have it to be a ſpherial, or ſpheroidical; together with ſome hints, tending to ſhew the uncertainty of the truth of the *Newtonian* phyſical part of aſtronomy, &c.

PROPOSITION I.

To enumerate the various opinions of the earth's figure ariſing from the bare view of a part thereof, when looked at by perſons ignorant of Geometry and Geography.

1. ALL thoſe who know nothing of geometry and geography, who have been moſtly uſed to views of the Land and waters free from mountains,

hills and valleys, naturally take the ſurface of the earth to be a broad and vaſtly extended plain, circularly running every way all round from him who is placed in its centre, to the very heavens, whereat he thinks the circumference of the circle terminates, and falls into what is called the ſenſible horizon, or bound of viſion: and ſo the ignorant inhabitants of a very large plain would naturally think the ſurface of the earth to be a circle, and from thence might be apt to take the earth itſelf to be a cylinder or ſolid, made by the motion of the horizontal circle aforeſaid moving downwards, perhaps to an indefinite depth. And that amongſt the antients there have been people of ſuch opinions, may be ſeen at the beginning of *Varenius*'s geography.---*Anaximander* thought the earth to be of a cylindrical figure, as is related by *Peucer*; but *Varenius* does not believe him; becauſe *Anaximander* attempted the meaſure of the earth, and was a tolerable aſtronomer for the age he lived in, as having erected at *Lacedemonia* an *heliotropia*, in which the top of the *Gnomon*, by its ſhadow, ſhewed the days of the Equinox and Solſtices. But this to me is not a ſufficent argument to ſhew the falſehood of *Peucer*'s aſſertion, with regard to *Anaximander*'s opinion of the earth's being cylindrical.——*Heraclitus* is ſaid by ſome to be of this opinion, and by others, that it was a deep concave ſolid.----*Lactantius*, St. *Aguſtin*, and ſome others of the antient fathers, confidently aſſerted the earth to be a ſolid, whoſe upper ſurface is flat, infinitely extended downwards, and firmly fixed upon its baſes.---*Ariſtotle* in his 2d. book of the heavens, chap. 13. relates many odd opinions that the antients had of the earth's figure.

2. The ignorant inhabitants of a mountanous country, as Switzerland or Wales for inſtance, who have never gone 20 miles from the place of their birth, muſt have very confuſed and different notions of the figure of the ſurface of the earth according to the mountains and valleys they are moſtly amongſt and uſed to look at——the earth's figure to theſe muſt ſeem vaſtly irregular: even ſo much,

that no words of theirs could expreſs the ſame, nor no means could be uſed by them to convey their notion of the earth's figure to another, who was deſirous to have their opinion about it, unleſs it were carved or painted.——Now ſince a man is apt to judge of the ſhape of a thing from his ſight, and becauſe his ſight cannot take in a large object at one view; that is, ſince he can only ſee a part of that object at one view, and it appears to him of ſuch a ſhape, he is apt to think all the other parts of that thing to have that ſame ſhape; and thence concludes the whole to be of the ſame figure as each of the parts. But this ſort of proof is oftner falſe than true, and leads men into a multitude of errors.

If a ruſtick lives on an extenſive plain, he thinks the ſurface of the earth to be ſo. If he lives in or near to a very large and deep valley, he thinks the ſurface of the whole earth to be one vaſt hollow or valley; and if he lives on the top of a very extenſive high mountain, he takes the ſurface of the whole earth to have a convex figure.

PROPOSITION II.

The earth is really of a very irregular figure, or ſolid polyhedron, whoſe ſurface conſiſts almoſt of an infinite number of plains and curve ſurfaces, viz. flats, hills and valleys, rivers, ſeas, &c. and theſe of various ſhapes and magnitudes, between themſelves.

THE truth of this propoſition eaſily appears in its full extent to thoſe who have travelled into many foreign countries; for what do they ſee in their paſſages from place to place but plains, mountains, hills, valleys, rocks, woods, rivers iſlands and ſeas? In ſome places of one ſhape, magnitude, height, and depth; and in other places ſuch as have different ſhapes, heights, depths and magnitudes.——And that the propoſition is partly true, every man will find who has travelled but a few miles out of his own country, and looks at the hills, valleys, flats, &c. that lie before his eyes, and make a part of the ſurface of the earth.——If the obſerver be upon an high hill in an hilly-country, he may in a clear ſunſhiny day view the country from thence 20 or 30 miles round, and behold with his eyes the irregularities of the earth's ſurface within that compaſs. If he looks at the ſea, eſpecially in calm weather, it appears flat and ſmooth; and ſince ſome or all theſe different views of flats, ups and downs, are really found to happen to every man at whatſoever part of the earth he has made obſervations, it neceſſarily follows that the earth's ſurface is really and vaſtly irregular, and very remote from a plain or any other ſimple figure. The whole ſeems to be made up of the continued mixture of the parts of all figures, both plain and curve-lined, from the moſt eaſy and ſimple few that are known and defined by geometricians, to the moſt difficult and compound; many whoſe natures are quite unknown, and conſiſt of no regularlity or ſimplicity worth notice, or a man's labour to deſcribe and give names to.

PROPOSITION III.

The Earth, viz. the ſea and land is doubtleſs of a roundiſh figure, that is, it is a ſurroundable ſolid. But we have no ſufficient proofs that this ſolid is a ſphere, or ſo near one as to be taken for a ſphere without any ſenſible error.

IT is plain the earth is of ſome circumſcribed and bounded ſolid figure, whoſe ſurface perpetually declines or falls lower and lower every way round about, from any place or part of one country that a man travels or ſails from, till he is got to other countries at the greateſt diſtances from the place he firſt ſet out from, and ſtill going directly on from any of theſe moſt diſtant places, he he will again continually approach to the place he firſt ſet out from. I ſay this is manifeſt from all obſervations of ſeamen and travellers.

1. Whoever goes or ſails ſouthwardly in our northern hemiſphere, will obſerve the ſun at noon to be higher and higher, each day the further he goes or ſails, till at laſt, when he comes to the equator, he will ſee the ſun at noon directly over his head, on the equinoctial day; and ſtill proceeding ſouthwards, he will find the ſun's meridian altitude each day to decreaſe more and more the farther he goes, till at length it will be the leaſt poſſible. The ſame alſo is obſerved of the fixed ſtars. If we go from any place, in the northern hemiſphere northwards, the northern ſtars will appear more elevated, and the ſouthern ſtars

more depreſſed, and the ſouthern ſtars that cannot be ſeen at all in theſe northern parts of the earth, will be ſeen by going ſouthwards, and the more elevated the farther you go or ſail.——In a word, there can be no diſpute about the roundiſh figure of the earth and ſea. The multiplied experience of all the ſeamen and travellers of theſe latter ages moſt fully confirm it. But the following proofs, as given by moſt of the geographers and aſtronomers, of the ſpherical figure of the earth, I take to be fallacious and inſufficient. Theſe ſay it is evident the earth, that is, the ſea and land, make but one ſpherical body, from eclipſes, eſpecially thoſe of the moon, which are cauſed by the ſhadow of the body of the Earth being interpoſed between the ſun and moon; for, ſay they, ſince this ſhadow does fall upon the moon all ways, and upon every ſide circular, and and ſo appears to us; it is manifeſt from opticks, that the earth from whence it proceeds, is a ſpherical body.

But I do not take this to be ſound reaſoning, nor ſtrictly true. ——The thing to be proved, being here taken for granted, *viz.* that the earth's ſhadow appears to us to be circular; whereas in truth, it only appears to be roundiſh, or of a figure that might proceed from the ſhadow of the earth, if its figure were an ovalar ſolid, or a regular polyhedron——If the earth be a ſphere, the ſhape of its ſhadow caſt upon the moon, will not be always circular. For the interſection of the cone of the earth's ſhadow caſt upon the body of the moon, will not be a circle, but another kind of oval, unleſs the moon be a ſphere, and the centre of it falls into the axis of the cone of ſhadow. ——There is no forming a true judgment of the preciſe figure of a body by the appearance of the figure of the ſhadow of it, caſt upon a very remote body. For, as I have ſaid, the ſhadow of a regular polyhedron may appear round or circular, as to ſenſe, as well as that of a ſphere, and ſo may the ſhadow of a ſpheroid, if that ſpheroid has not its two axes very unequal. Even the ſhadow of ſome irregular polyhedrons, may ſenſibly appear to be ſpherical, when caſt upon very remote round bodies. ——— *Lucretius* tells us, that ſquares ſeen at a diſtance do ſometimes appear to be round, which I myſelf have experienced in ſome caſes. Granting this, regular polyhedrons of many ſides, muſt be more readily allowed to do ſo, becauſe theſe approach nearer to circles than ſquares do. A ſphere ſeen at a diſtance will (when there is no irregular refraction) always appear to be perfectly circular. But on the contrary, other bodies, not ſpherical, may ſometimes appear to be round.——Refracting mediums of different denſities, wherein the eye and the bodies are placed, will cauſe an alteration of the apparent figures of bodies. I have often ſeen outward bodies, through bad glaſs windows, much altered and diſtorted in their ſhapes. Sometimes the ſun and moon at their riſing and ſetting do appear to be of ovalar figures. —— A ſtrait ſtick partly in the water and partly in the air, will appear to be crooked.——If the ſun or moon were in ſhape of a regular polyhedron, for inſtance, a dodecahedron or icoſahedron, I am almoſt certain they would appear to our eyes here on earth to be circular.

2dly. The geographers and aſtronomers ſay, "it could not be determined when "and in what place an eclipſe of the ſun "ſhould appear, or where not, if the figure "of the earth were not known (an eclipſe "of the ſun being cauſed by the interpoſi-"tion of the moon between the ſun and "thoſe places of the earth where it ap-"pears eclipſed.) But as the places where "theſe eclipſes happen, and were not, are "determined by aſtronomers, upon the "ſuppoſition of the ſurface of the earth's "being ſpherical; it muſt follow from "thence that the earth is ſpherical."-- Now this concluſion is as uncertain as that above, for the earth may be a regular polyhedron of many ſides, and ſo its ſhadow not conical, and yet the times when, and places where an eclipſe of the ſun will happen, may be computed as near the truth, as if the earth was the ſphere wherein that polyhedron was inſcribed. If the earth were an icoſahedron, as to figure, inſcribed within a ſphere, or that ſphere itſelf, I dont ſee what difference there would be in the times and places of a calculated eclipſe of the ſun, or whether the real times and quantity of the eclipſe, in the one caſe or the other would be ſo different, as to be obſerved.—— Therefore this proof of the earth's being a ſphere, becauſe the times when and the places where the ſun's eclipſe will happen, as found by calculation, are ſeen to be nearly true by experience, is not to be relied on.

3dly. It is ſaid the earth is ſpherical, "because eclipſes of the moon happen "ſooner at the places of the earth, "ſituated more eaſtwarly, than at thoſe "ſituated more weſtwardly; and that "the times of their happening are pro- "portionable to the diſtances of the "places that lie eaſt and weſt from one "another."

“ another.”—But this laſt aſſertion of the proportionality of the times and diſtances, I am doubtful of; nor where and by whom experimental proof of it has been actually made by the exact meaſurement of the diſtances of the places lying eaſt and weſt from each other, without which there is no certain dependance upon that aſſertion.----Moreover, it is taking the thing for granted to ſettle and lay down the longitudes of places by the eclipſes of the moon, and thoſe of Jupiter's Satellites, as moſt of our aſtronomers and geographers do.----Theſe make the eaſt and weſt diſtances of places to be proportionable to the times of the ſame eclipſe, either the beginning, end or middle of it, happening ſooner or later, at the one place than at the other, without knowing whether the real meaſured diſtances of thoſe places be proportional to thoſe times. And hence it has happened, that different geographers, truſting to their own obſervations of the times of the beginning, middle and end of an eclipſe of the moon, at different places, have put down in their tables, globes and maps, the ſame places, as having different longitudes when they ſhould not. Whoever caſts his eyes upon different maps, will ſee too much of this diſagreement in the longitudes of the ſame places.

4. Another ſophiſtical argument, as I take it to be, brought to prove the earth to be ſpherical is, " that the differences " of the latitudes of places under the " ſame meridians, are always pro- " portional to the diſtances of thoſe " places"——Here again the thing is taken for granted; becauſe we cannot be certain it is ſo, but by the actual meaſurement of the diſtances of the places; nor have men ever yet done ſo much of this buſineſs, and ſo truely withal, as to confirm the truth of the above aſſertion about that proportionality in all caſes that may be neceſſary—At land it is no eaſy matter to meaſure and aſcertain the diſtances of two remote places, lying under the ſame meridian or not, ſufficiently exact.——Alſo the latitudes of places can be no otherwiſe found out

than by taking the meridian altitudes of the ſun or fixed ſtars, thereby to obtain the difference of the latitudes. The ſun's declination, and ſtars right aſcenſion, muſt be known at the time of theſe obſervations; and all errors from the inſtruments themſelves, the obſervations, the ſun's declination, and ſtars right aſcenſion, will cauſe errors in the latitudes determined by them--*Copernicus* makes the ſun's greateſt declination 23° 28'---*Tycho Brahe* 23° 31'. Mr. *Wright* in the year 1600 at London, by many diligent obſervations of his own, with a quadrant of more than ſix feet radius, makes it to be 23° 31'.--- Mr. de la *Hire* will have it to be but 23° 29'; almoſt every body elſe now a days make it to be 23° 30': and which of all theſe come the neareſt to the truth, I believe no man can tell. Add to this, that ſome make the ſun's greateſt declination to be unalterable, and others will have it to be mutable.

PROPOSITION IV.

To confute, or at leaſt leſſen the certainty of the truth of the aſſertion of the geographers, that the higheſt mountains and deepeſt valleys have no ſenſible proportion to the ſemidiameter of the earth, and that theſe do no more leſſen the ſpherical figure of the earth than ſmall unavoidable irregularities in the fabrick of an artificial globe, do leſſen its perfect and geometrical roundneſs.

AS theſe geographers know well enough that the high mountains and deep valleys which we ſee with our eyes, and are to be found in all parts of the world, moſt evidently and rigidedly ſpeaking, muſt be objections to what they ſay about the perfectly ſpherical figure of the earth, they endeavour to get clear of this by ſuppoſing their heights and depths to be but ſmall. But, all the proofs that I have ſeen appear to me, too ſcanty and uncertain to be entirely relied upon.——*Varenius* at chap. 7. of his general geography, will have the mountain, called "Pico de "Teneriff to be the higheſt mountain in "the world, and by experience, ſays he, it hasbeen found to be only one mile, or a mile and a half in height." But an ingenious friend of mine, Mr. *John Eden*, a ſecretary to the late Sir *Charles Wager*, did meaſure its heighth as he told me, and found it to be near three miles. Now, why this mountain ſhould be taken for the higheſt in the world, I cannot realy tell; it being what no man can truely affirm, and experimentally know: here faith is to ſupply proof, and any man is left at liberty to believe as he pleaſes.——If *Pico de Teneriff* be the higheſt ſingle mountain that has been taken notice of becauſe of its ſhape, ſituation, and great apparent altitude,

titude, yet there may be ſome parts of the vaſt chains of mountains that are to be found running through the midland parts of the world, higher than that, or any other ſingle mountain that has been taken notice of by any body, either the ancients, or moderns, and whoſe altitudes have not been taken by inſtruments or any artificial trigonometrical method. We read in hiſtory of ſome mountains of prodigious heights, even 60 or 70 miles. But ſome take theſe relations to be falſe, for no other reaſon perhaps, than becauſe ſuch great heights would be obſtacles to ſome favorite hypotheſis of theirs.

Some aſtronomers tell us of mountains in the moon, whoſe heights far exceed any of thoſe upon the earth that have been meaſured.——Others again mention mountains in the planet *Venus* much bigger than thoſe of the moon: it is ſaid in the memoirs of the royal academy of ſciences at *Paris* for the year 1710, "Mr. *de la Hire* with a teleſcope of 11 "feet long diſcovered mountains in

" *Venus* greater than thoſe of the moon, " whoſe diſk at the ſame time appeared " through the teleſcope to be thrice as " big as that of the moon ſeen with the " naked eye."——A part of the moon's ſurface ſeen even with the naked eye, when in her third quarter, appears in the morning to be very irregular and jagged, and through a teleſcope it muſt needs appear to be much more diſtinct, irregular and magnified; and ſcarcely any body doubts but that ſuch parts of the moon are mountains and valleys, very high and deep, becauſe they can be ſeen by us who are at ſo great a diſtance from the moon.

Since therefore the moon's ſurface has ſuch very remarkable high parts and vaſt cavities on it, and Venus likewiſe has bigger, and becauſe many aſtronomers, with *Kepler*, make the earth to be greater than Venus, and Venus greater than the moon, I think it muſt be granted that the prominent parts of the earth ought to be bigger than thoſe either of the moon or Venus.——*Ricciolus* in his almageſt nova, ſhews how to find the altitude of one of the moon's mountains, and makes it to be nine miles. See alſo Dr. *Keil's* aſtronomical lectures. If this be true, the moon's ſpherical figure muſt be as much diſtorted by it, as one of the earth's mountains of above 40 miles in height would disfigure the earth's roundneſs, ſince according to ſome aſtronomers the ſemidiameter of the moon is to that of the earth, as 113 is to 506.

Again I do not think the magnitudes and figures of any material compoſitions do continue the ſame unleſs it be ice or glaſs, or ſome other vaſtly compact ſubſtances; but are always altering, although the times neceſſary to bring this about in ſome ſpecies of beings, are comparatively ſpeaking very great, for nature ſeems to delight in variety and change. Therefore why ſhould it be wrong and unreaſonable to ſuppoſe, that ſome famous mountains which have been taken notice of many ages ago, were higher then they are now, and alſo different in ſhape, at the end of ſome thouſand years. Are not mountains firſt generated, and gradually grow up to their greateſt magnitudes, and altitudes, in a vaſt courſe of years, and then become leſs and leſs.——And may we not even go farther, and ſuppoſe the figure and magnitude of the whole earth, or its parts, not to be permanent and everlaſtingly the ſame?——Hence it is eaſy to account for the relations given by the antients of the great heights of ſome mountains, which are now taken to be fabulous, by ſuppoſing thoſe mountains to have decreaſed in their altitudes, and beſides altered in their figures too.----In Dr. *William Gilbert*'s phyſiology at page 176, it is ſaid that in the reign of *Ogygis* king of *Thebes*, many ages before *Ariſtotle* was born, the planet Venus changed her colour, figure, magnitude and courſe.——We have many inſtances in hiſtory of the changes and alterations that have happened to the parts of the earth; where there was dry land once, there is now water, and where water once there is now dry land. *Herodotus* relates, that the dry land of *Egypt*, which now extends from *Memphis* to the mountains of *Æthiopia,* was all ſea. ——*Ariſtotle,* and others of the *Greeks,* tell us of many antient flouriſhing places that are now covered with water; and contrariwiſe, the famous iſlands of *Rhodes* and *Delus,* which formerly had their foundation in the ſea, by the force of the waters, are now ſunk and buried under water, and not to be ſeen at all. —— *Sicilly* was once joined to *Italy.* See much more of theſe admirable and great changes, in the 18th chapter of *Varenius*'s geography, beginning at page 204 of Dr. *Jurin*'s edition.

I could ſay much more on this ſubject, but that it is foreign to my purpoſe; theſe hints are enough as I think; for whether the heighths of ſome ſingle mountains that have been meaſured, be more or leſs then three miles, I am much inclined to believe *the heighths of ſome lands and chains of mountains above the levels of the ſea ſhores, do much exceed three miles, as I think will be evident enough from what follows.*

Every body almoſt knows many parts of the land that appears to be flat, and even, are not of the ſame heighths; ſome being lower than others, eſpecially the ſea ſhores; and that the heighths of the midland parts encreaſe from the ſea ſhores, to the ſpring heads, is proved by the flowing of the water from thoſe ſpring heads to the ſea ſhores. For that part of the land to which the water flows, is certainly lower than that from which it runs.—And it ſeldom happens that the ſpring-heads or rivers take their origin any where but at the inland places that are very remote from the ſea. And thus the ſpring heads or rivers, are higher than the ſea ſhores whereat thoſe rivers run into the ſea.

It is by ſome allowed that the deſcent of rivers at a medium is about one foot in two hundred, (as *Varenius* ſays in his geography) and the river *Nile*, for example, takes its riſe at the lake *Zaire*, in ten degrees of ſouth latitude, and runs almoſt directly north to the *Canobicum Oſtium*, in the thirty-firſt degree of north latitude, taking a courſe from its ſpring head to the ſea of forty-one degrees, which makes above 2850 *Engliſh* miles, allowing 69½ eighth miles to each degree, and taking in the bendings of that river, its courſe may be eſteemed at 3000 miles, and allowing 1 mile of deſcent for every 200, the height of the lake *Zaire* will be 15 miles above the ſea ſhore, whereat the river *Nile* runs into the ſea, and the tops of the mountains near that lake will be yet higher.

In like manner the river *Niger* in *Africa*, has its ſpring in 11 degrees of north latitude; one of its mouths is in the ſame latitude as its ſpring, and its moſt remote latitude is 15 degrees; its courſe is weſtwards, and it runs under ground for ſome ſpace, and then riſes again; its courſe is ſaid to be about 600 *German* miles. But it will be leſs if all its windings are omitted, and more if theſe be taken in, ſo that its true courſe may be as long perhaps, as that of the *Nile*; and therefore its ſpring head will be 15 miles above the ſea ſhore where its mouth is, if for every 200 miles of its courſe be allowed one mile of deſcent—. The courſe of the river *St. Lawrence* in north *America* is about the ſame length as that of the other two rivers abovementioned. And ſo the lands about the ſpring head of this river will be 15 miles above the lands near the entrance of this river from the ſea.——There are ſeveral other rivers that run through vaſt tracts of land, and the heighths of the lands near their ſprings, muſt neceſſarily be much greater than thoſe of the maritime places whereat they diſcharge themſelves into the ſea.——I don't pretend to affirm this to be an unexceptionable proof, becauſe of the uncertainty of the two premiſes upon which it is built, *viz.* that the courſes of the rivers are ſo long as is mentioned, and that they have at a medium a deſcent of one mile in every 200.——But yet this is as good proof, as are many of the poſitions of thoſe whom I have undertaken to oppoſe.

Again, many are of opinion the *Atlan-*

tick and *Indian* oceans are higher than the *Mediterranean* ſea.——Others will have the *Indian* ocean between *Africa* and *India*, to be higher.——It is related in hiſtory, that *Seſoſtrus*, *Darius*, and other antient Kings of *Egypt*, had a deſign to make a chanel from the *Red ſea* to the river *Nile*, by means of which ſhips and veſſels might paſs out of the *Indian* and *Red ſea* into the *Mediterranean* ſea. But the deſign was never put into practice, becauſe ſkilful perſons who made trial, found the *Red ſea* to be much higher than the land of *Egypt*.——Other antient Kings and governors of *Egypt*, and *Turkiſh* emperors, ſeveral ages ago, had a mind to dig a chanel through the *African* neck of land which ſeparates the *Mediterranean* and *Red ſea*——But this deſign was laid aſide, becauſe being told it was found the *Indian* and *Red ſeas* were higher than that of the *Mediterranean* ſea. They feared leſt the waters running out of the *Red ſea* into the *Mediterranean* ſea ſhould overflow the ſhores of thoſe countries, eſpecially *Egypt*, which by all writers is allowed to have a low ſituation.

Although it is very reaſonable and probable to ſuppoſe, that ſome ſeas are naturally higher than others, as well as we are ſure ſome parts of the land are; yet it is not eaſy to obtain certainty about this by actual meaſurement, or levelling either of the greater heights of ſome inland places above others, how high one ſea exceeds that of another, or one river.——I take *Oxford* to be higher than *Glóceſter*; the river *Thames* than the *Severn*: the midland flats and plains in *England* than thoſe nearer the ſea. The method of doing this thing by the riſing and falling of the mercury in the barometer appears to be very feaſible, eaſy and practicable. But it has been found by experience to be fallacious, and not to be much depended upon. See the *Hiſtor. del. acad. Roy.* 1703, 1705. and our philoſophical tranſactions.

In a word, ſince *Varenius* and his followers, do all agree that the ſources of rivers are higher than their mouths where they run into the ſea, and accordingly the inland places whereat they ſpring are higher than the lands near their mouths; and becauſe the quantity of that exceſs has been already ſhewn to be 15 miles, upon the ſuppoſition that the deſcent of every 200 miles of the three rivers before-mentioned, *viz.* the *Nile*, *Niger*, and the river *St. Lawrence*, to be one mile; and that the courſe of each of them is 3000 miles:——I ſay, granting the truth of theſe two things, it is evident ſome lands are 15 miles higher than others, which muſt neceſſarily too much ſpoil the ſpherical figure of the ſurface of the land and waters: nay, if inſtead of 15 miles, ſome lands had been found to be but 10 or 12 miles higher than others; this would be too much for thoſe who aſſert the ſpherical figure of the earth: the ſame may be ſaid of the altitude of the ſeas, if they be conſiderably different.

PROPOSITION V.

There is no-body can tell really to which of the round figures, viz. *a ſphere, ſpheroid or other ſolid, the figure of the earth approaches the neareſt.——The arguments of all who have treated of this being very inſufficient and doubtful, as I think.*

1. NO certain knowledge of the true geometrical figure of the earth can be otherwiſe obtained, than by much more actual meaſurement on its ſurface than has been ever yet, or perhaps ever will be done.——Who can find the figure of one meridian, or north and ſouth ſection of the earth through the center, but by the actual meaſurement of many conſiderable diſ-

tances under that meridian?——I don't read of any meaſurement under the ſame meridian at two different places thereof, except in *France* there were two, by different perſons at different times; whereas there ſhould be 10 at leaſt, if not more. If this were done, and the figure of the meridian was really not a circle or ellipſis, or other curve line of the next higher geometrical order, I don't perceive how its figure could be had by any meaſurement at all.—— If the figure of one meridian was found to be nearly circular or elliptical, it does not follow that the figure of all the reſt of the meridians muſt be equal circles, or ſimilar and equal ellipſes.—— Many actual experiments under ſeveral diſtant meridians muſt be firſt made to prove this. But there has been no ſuch numerous and ſufficient meaſurements ever yet made.——Therefore it remains doubtful whether the meridional ſections of the earth be all equal circles, or ſimilar and equal ellipſes, which they muſt neceſſarily be, in order to verify the ſpherical or ſpheroidical figure of the earth.

2. Thoſe who argue for the ſpherical figure of the earth, do in ſome ſort take it for granted, that the earth is ſpherical without ſufficient proof, as I have ſhewn in the laſt propoſition; and founding their reaſoning upon this, take all the meridians to be equal circles, and all the parallels to be decreaſing circles from the equator to each pole; and ſo by meaſuring one ſhort diſtance under one meridian or parallel circle; by means of that and the difference of the latitude of the places whereat they began to meaſure, and the place to which they meaſured, think the reſult will give them the circumference of the earth, and the length of a degree. But this is founded upon uncertain hypotheſis, being taking that for granted which ſhould have been firſt proved, *viz.* that the earth is ſpherical; then indeed all would be a right, and one exact meaſurement under the ſame meridian of ſufficient extent would be enough to determine the length of a

degree, by means of the different latitudes of the places, whereat the meaſurement began and ended.

3. Since the geographers and aſtronomers differ in their Sentiments concerning the earth's figure, it is plain ſome of them muſt be wrong. If Mr. *Huygens* and Sir *Iſaac Newton* are right in their arguments to prove the figure of the earth to be a flat ſpheroid, then will all the antients and their followers be wrong, in their aſſertion of the nearneſs of the figure of the earth to a ſpherical figure.

4. If the figure of the earth be the neareſt to a flat ſpheroid, that is, one generated by the rotation of a conick ellipſis about its leſſer axis; (which is the axis of the earth) Mr. *Caſſini* and his followers muſt be miſtaken, in ſuppoſing the figure of the earth to be a long ſpheroid, generated by the revolution of a conick ellipſis about its longer or tranſverſe axis.

PROPOSITION VI.

To enumerate the uncertainties, and hypothetical aſſertions of thoſe who ſuppoſe the earth to be in the figure of a flat ſpheroid.

THE firſt broacher of the earth being of the figure of a flat ſpheroid, was Mr *Huygens*, and afterwards Sir *Iſaac Newton*; and both theſe ſuppoſe, " the " earth at firſt to have been in a fluid ſtate " at reſt and ſpherical; but afterwards " being put into motion about its axis, " its ſpherical figure became altered " into a flat ſpheroid, higher in the " middle than at either of the poles, " by reaſon the centrifugal force was " greater in the middle than it was to-

" wards the poles; and beſides, *Newton* " further ſuppoſes, the figure of the " earth ſhould be a flat ſpheroid, to hinder the waters running from the polar parts from overflowing the lands, " whilſt the earth revolved about its " axis." But this is mere ſuppoſition, the truth or falſehood of which no man can tell.——Who knows what the figure of the earth was in the beginning, or of what matter it conſiſted, or how that matter was diſpoſed and varified in the internal parts of the earth, to hinder the pernicious effects of the waters overflowing the land, without having recourſe to the neceſſity of the lands being higher at the equator than it is at the poles, when the earth was put into motion about its axis.——Sir *Iſaac Newton*'s diſcourſe (as well as that of others) upon this ſubject is full of doubts and uncertainties; and when I read it, many years ago, I was far from being convinced of the truth of his concluſions.——He takes the earth's motion about its axis to be equable without ſufficient proof that it is ſo.———He ſuppoſes the equator to be a circle, and all the parallels of latitude to be circles without proof.———He takes the earth's axis to be a fixed line within it, or a line moveable about its centre, with a ſmall angular motion; when for any thing he knew to the contrary, that axis may have ſome other very irregular motions.

Again, becauſe ſalt water has been found by experience to be heavier than freſh water, and the ſea water in the hot climates has been found by experience to be more ſalt than the ſea water in the more northern parts, and the high latitudes, I queſtion whether the greater weight of the ſea waters under the equator and near to it, above the ſea water nearer the poles, may not be ſufficient to hinder the polar waters from running towards, and riſing up at the equator, by the centrifugal force during the earth's motion about its axis, without having recourſe to the neceſſity of the land and waters being higher at the equator than it is at the poles to hinder it.——It is even thought by ſome, that ſalt is the cauſe of the coheſion of the particles of bodies, and the cauſe of gravity too. Whether the Iſland of *Ormus* be not all ſalt, and the earth itſelf in thoſe parts near the equator, ſalt.——Moreover, who knows whether the gravity of the parts of the earth at all equal diſtances from the center may not be variable.

There is another reaſon given by Sir *Iſaac Newton*, and all his followers, ſupported by ſome experience it is true, but not enough as I think, *viz.* ‘ He will ‘ have the earth to be a flat ſpheroid, be‘ cauſe ſeveral *Frenchmen*, and the late ‘ Dr. *Halley*, finding by experience that ‘ pendulum clocks at ſome iſlands and ‘ places near the equator went ſlower ‘ there than at *Paris* or *London*; he from ‘ thence concludes theſe iſlands and ‘ places to be higher or more diſtant from ‘ the center of the earth, than is *Paris* ‘ or *London*.’ But the differences in the lengths of the pendulums at *Paris* or *Lon-*

don from thoſe at the equatoreal places, are two ſmall, and the experiment confined to too few places, for a ſufficient foundation to build ſuch a truth upon.—Nay, Mr. *Picard* had not found his pendulum clock to go faſter in *Denmark* that it did at *Paris* nor even than it did in the ſouthern parts of *France* ; that is, the pendulum kept to the ſame length, altho' the difference of latitude of both places between *Copenhagen* and the ſouthern parts of *France*, whereat the clock was carried, was more than 10 degrees.——If this be true, the *Newtonian* proof of the flat ſpheroidical figure of the earth cannot exhiſt; for according to the *Newtonians* opinion and hypotheſis, the going of the clock at *Denmark* ought to be faſter than at *Paris* or the ſouthern parts of *France*; that is, the force of gravity leſs in *France* than it is in *Denmark*, and accordingly the land in *France* higher or more diſtant from the centre of the earth than it is in *Denmark*.——It will be eaſy for thoſe who doubt of the truth of Mr. *Picard*'s experiment, to try whether it be ſo or not; as to me, I have as much reaſon to believe it, as the *Newtonians* have to believe the *French* mathematicians clocks that went ſlower near the equator than at *Paris*.—But the *Newtonians* will have the unequal goings of a clock in different latitudes to be only cauſed by the lengthening or ſhortening of the pendulum, or of an increaſe or decreaſe of the force of gravity at two places of different latitudes; whereas this may happen from the greater or leſs denſity of the air, or of ſome particles of moiſture, &c. in it, conſiderably affecting the times of the clock's going, when the length of the pendulum is not altered; for if a pendulum vibrates in a denſe fluid, as water ſuppoſe; the time of the vibrations will be leſs than the time of its vibrations in the air, by reaſon of the greater reſiſtance in the bob and wire of the pendulum, in the one caſe than in the other, for where the reſiſtance is the greater, there the retardation is ſo too——Beſides, I knew by experience that a pendulum clock did once go too faſt by adding a weight to that of the clock much more than ordinarily, altho' the pendulum by this means did ſwing larger arches than uſual, and ſo by the theory of the doctrine of the motion of pendulums, it ſhould have been longer performing each of its vibrations——Hence the theory of theſe things is not to be depended upon, unleſs backed by experience: alſo heat and cold will affect the going of a clock.

Hence it cannot be certainly known by means of pendulums or clocks, whether one place be higher than another; ſince the different denſity, coldneſs, moiſture or elaſticity of the air, will make the clock go ſlower or faſter, as well as the greater or leſs heighths of the places whereat the clock ſtands. Indeed if the pendulums of the clocks were to vibrate in vacuo, or in air of the ſame denſity and heat, the force of this objection may be avoided perhaps.—— I really think the great moiſture of the air of the iſlands and places in the neighbourhood of the equator whereat the *French* mathematicians many years ago, found their clock to go too ſlow, might have had ſome effect in producing this ſlowneſs.

PROPOSITION VII.

To point out ſome of the doubts and inſufficiencies contained in the treatiſe, entitled the figure of the earth determined from obſervation made by order of the French *king at the polar circle in the year* 1736, *and that the true figure of the earth is neither determined by theſe* French *mathematicians nor by any body elſe.*

1. THESE gentlemen will have the figure of the earth to be a flat ſpheroid, becauſe they meaſured under the

artick circle a length of 57437·9 *French* toiſes upon the meridian, and by obſervation found the difference of the latitudes of the two places at the beginning and ending of their meaſurement, to be 57 minutes, and 27 ſeconds.——Now I ſhall willingly grant both the truth of this meaſurement, and difference of latitude. Then ſay theſe gentlemen "the "length of a degree of the meridian "*(of Tornea)* that cuts the polar circle, "will be 57437 toiſes, exceeding that of "Mr. *Picard's* between *Paris* and *Amiens*, "*viz.* 57060 toiſes by 377 toiſes."——Now I ſhall not diſpute the truth of this; but what does it really prove? why no more than that a degree of the meridian at *Tornea*, was 377 toiſes greater than a degree of meridian from *Paris* to *Amiens*; it cannot in the leaſt be ſaid the figure of the earth is a flat ſpheroid, becauſe of this ſingle inſtance——Many other ſuch like trials ſhould be made at different places under the meridians of the artick circle, and under the meridians croſſing the parallels of the leſſer latitudes,

tudes, if poſſible, all of which ſhould be found to conſpire in the ſame thing, before the earth's true figure can be affirmed to be a flat ſpheroid.

Theſe *French* gentlemen fall foul on Mr. *Caſſini*, for aſſerting the figure of the earth to be oblong; becauſe he found by the actual meaſurement of about 7½ degrees of a meridian in *France*, that each of theſe degrees encreaſed a ſmall matter from north to ſouth.——— If this be a fault, the *Newtonians* are equally as wrong, in aſſerting the earth to be a flat ſpheroid from two few meaſurements actually made, and finding the length of a degree more northwardly to be greater than that more ſouthwardly. ———Mr. *Caſſini*'s meaſurements may be right, but his long ſpheroid will not follow from them: ſo may the *Newtonian* ones be true; but their flat ſpheroid cannot be fairly obtained from them neither; even *Newton* himſelf at page 381 of his *princip.* makes uſe of the *Caſſinean* meaſurement, to prove that the earth is higher at the equator than it is at the poles, by $17\frac{1}{6}$ miles: and at page 383, *Newton* gives a table, ſhewing the toiſes in one degree of the meridian for every 5 degrees of latitude; by which, ſays he, it appears, the inequalities of degrees are ſo ſmall, that in the buſineſs of geography, the figure of the earth may be taken to be ſpherical. But in the next page, ſays *Newton*, this table and the ſaid exceſs of $17\frac{1}{6}$ miles, will not hold good, if the earth does not conſiſt of uniform matter; for if it be a little denſer at the centre than it is at the ſurface, the land at the equator will ſtill be higher than it is at the poles, and the difference of the degrees of the meridian greater than they are in that table. And at page 386, he will have the matter of the earth at the centre, to be a little more denſe than it is at the ſurface.—Therefore, *Newton*'s table aforeſaid, is only founded upon mere hypotheſis, and of no real uſe at all, either in geography or navigation, as I think.

Again, how uncertain muſt *Newton's* ſuppoſed equal denſity of the earth be, and every thing founded upon it.——— *Varenius* at prop. v. chap. i. of his general geography, truely, I think, ſays it is uncertain how the parts of the earth from the ſurface down to the centre are diſpoſed.———He tells us, ſome think there is water below about the centre of the earth. But he will have it to be more likely there is land.———*Gilbert* takes the body of the earth to be one ſolid *magnet* or *load-ſtone*; and thoſe parts of it which we come to by digging, and on which we live, and where all vegetation is performed, is a kind of cruſt or ſhell thereof.———*Deſcartes*'s opinion does not much differ from *Gilbert*'s; he will have the earth to conſiſt of three kinds of ſubſtances, the rareſt being about the centre; the next being denſe and opack, conſiſts of the moſt minute particles; and the third, whereon men have to do, is made up of leſs cohering particles.———But, ſays *Varenius*, there is ſcarcely any thing of certainty to be obtained in this; for in many ſubterranean places, there is flame and ſmoak ariſes from ſulphur, as is manifeſt from Baths.——Therefore, ſince there is ſuch a diverſity of opinions about the internal conſtitution of the earth, and it being utterly impoſſible for men ever to know by experience what it is, I may take the liberty as well as *Newton* to ſuppoſe the earth not to be uniformly denſe from the circumference to the centre, or even to be denſer upwards than it is at the centre, or to be more than a little more denſe at the centre than it is at the circumference.——But all this is uncertainty, mine as well as *Newton*'s.

Moreover, ſome may think there is a neceſſity for the parallels of latitude to be circles, in order for the earth to revolve equally about its axis. But this may happen, if the parallels of latitude be of different figures, provided the matter of which each of them conſiſts or runs through, be not of the ſame denſity all over.——For in this caſe the centre of gravity of each parallel, through which the axis paſſes, will only be different from the centre of each circular parallel of uniform matter; and from the experience we have about the different denſities of the matter we find in the uppermoſt parts of the parallels of latitude; ſome being found to be partly water, partly land, partly ſtone, partly clay, partly minerals, *&c.* it might lead one to think that the ſeveral parallels of latitude are not circles, but other curve lines, and ſo the earth itſelf to be neither a ſphere nor ſpheroid; and the centre of gravity, and that of magnitude of the earth, not both to coincide, as they muſt do in a ſphere or ſpheroid of uniform matter; according to *Newton* and others.———All heavy ſolids have a centre of gravity, but have not a centre of magnitude; there are but few of theſe laſt comparatively ſpeaking: the axis of the earth paſſes through its centre of gravity, let the earth be of what figure ſoever, and when the centre of gravity, and that of magnitude of the figure of the earth is one and the ſame, the revolution of the earth about its axis will not be naturally equable.

Altho' the meridians in the theory are ſuppoſed to be equal plane figures, with one center, and equally moving about one axis, yet we are not ſufficiently ſure by experience, whether the earth moves equably about an axis, or whether the meridians be all plane figures, and their peripheries, or the meridian lines on the ſurface of the earth, do every one of them tend directly forwards without any curvity towards the eaſt and weſt.——*Keplar* thought the earth's diurnal motion was not exactly equable, but that it ſlackened or encreaſed according to the earth's greater or leſſer diſtance from the ſun.——*Tycho Brahe* had alſo ſome ſuch ſuſpicion. Moreover, when Mr. *Picard*, a famous *French* aſtronomer, drew a meridian line in the obſervatory of *Tycho Brahe* at *Uraniburg* in the year 1672, he ſays he found it to be 18 minutes different from that of *Tycho Brahe*, which gave occaſion for ſome, as Dr. *Wallis*, to think the two poles of the world, and conſequently the meridians paſſing through them to be mutable. [ſee the philoſophical tranſactions, numb. 255, and the hiſtory of the royal academy of ſciences at *Paris*, for the year 1710.]

Now either *Tycho Brahe* or *Mr. Picard* muſt be miſtaken, or elſe the meridian line of *Uraniburg* muſt have ſome motion; but I have not ſufficient data to help me to know where the miſtake lies, or indeed whether there be any at all.

Hence the flat ſpheroidical figure of the earth cannot be fairly and undoubtedly deduced from the few meaſurements that have been hitherto made.——Nor are the other arguments ſufficient for this purpoſe brought by the *Newtonians* from the regular diminution of gravity from the poles to the equator, diſcoverable by the ſhortening of a pendulum that ſwings ſeconds; who can be ſure whether in every place of the ſame parallel of latitude the force of gravity is the ſame, and accordingly the length of a pendulum, that ſwings ſeconds in all the places of the ſame parallel the ſame too.—Theſe *French* gentlemen at page 205 of the *Engliſh* edition of the meaſure of a degree at the polar circle, in the year 1737, own "hitherto the only proof of this dimi-"nution of gravity towards the equator, "was from experiments made in places "near it, too little diſtant from each "other to found an unexceptionable proof upon." And they wiſhed that trial was made in the *Eaſt Indies* at the latitudes of *Cayenne*, *St. Domingo*, and "*Jamaica*, whether the ſaid diminution "was there the ſame as in *America*."--- Theſe *Frenchmen* did try the thing at *Pello* in the latitude of 66 degrees, and 48 minutes, (See the aboveſaid treatiſe) and found at that place, their clocks in one revolution of the fixed ſtars gained there upon their motion, 53 ſeconds of one revolution of the fixed ſtars.---- Wherefore they ſay the acceleration of the clocks from *Paris* to *Pello*, is 59 ſeconds.----Although there may be brought many objections to leſſen the veracity of this, yet I will grant it to be ſo, at the times theſe obſervations were made, at each place. But how do they know whether the ſame thing will be obtained at other places in the ſame parallels of latitude with *Pello* and *Paris*. Experiments of this ſort ſhould be made, before fair conſequences can be deduced, of unqueſtionable veracity about the inequality of the earth's equatoral and polar diameters.

Theſe *Frenchmen* make the earth to be flatter than *Newton* does, but *Newton*'s flatneſs is greater than Mr. *Huygens*'s. It may be ſomebody hereafter will have it to be flatter ſtill. Theſe gentlemen tell us of another table of the late Mr. *Bradley*'s, that ſhews the increaſe of gravity from the equator to the poles, founded upon experiments made with a clock at *Jamaica*, and upon the *Newtonian* theœrem, *that gravity increaſes towards the pole in a proportion nearly as the ſquare of the ſine of the latitude*.

tude. But ſay they, the acceleration from *Paris* to *Pello*, as deduced from this table exceeds what we found it to be, 4″. 5. Mr. *Maupertuis*, alſo in that book of the figure of the earth, gives another table of this ſort of his own, calculated upon the following problem of his, *viz. the lengths and latitudes of two degrees of the meridian being given, to find the figure of the earth*.---But his manner of reſolving this problem is from mere mathematical ſpeculation, difficult withal, obſcure, and partly liable to error.

For herein he firſt ſuppoſes the earth "as a ſolid, generated by the revolution "of a conick ellipſis upon its axis, "from which it very little differs as he "will have it: preſently after he ſays, "the ſpheroid of the earth is very little "different from a ſphere."----By this way of talk it ſhould ſeem, that the real figure of the earth is but very little different from a ſphere, which manner of expreſſion moſt certainly confounds and confuſes every perſon uſed to an accurate way of ſpeaking and thinking; for the words *very little* uſed in this ſolution, perplexes the reader, becauſe of the unlimitted magnitude, and makes him think, he may as well take a ſphere for the real figure of the earth as a ſpheroid. But if a ſphere be taken, the author's propoſition is already granted, and his way of ſolution is vain, and makes out nothing to the purpoſe.—His algebraick computation alſo has ſomething of a preſs fault in it. As I ſuppoſe, he ſays, it is $D=\frac{E-F}{3\,E\,\overline{\overline{S^2-Fs^2}}}.$ now granting this to be true, the next equation he gives ariſing from that is $D=\frac{E-F}{3\,E\times\overline{\overline{S^2-s^2}}}$ but this is not true, for every body knows $3\,\overline{E\,S^2-F\,s_2}$ is not equal to $\overline{3\,E\times S^2-s^2}$. therefore it is $D=\frac{E-F}{3\,E\,S^2-E\,s^2}.$ If it were of any moment I could point out more inaccuracies, in this ſolution of Mr. *Maupertuis*'s problem, but I don't think it worth while. Note, according to the Newtonians, the figure of the earth is not a ſpheroid generated by the motion of a conick ellipſis about its axis, but a ſolid made by the revolution of an oval, of the third order of geometrical lines, about it axis, as may be ſeen in Dr. *Keil's* ſolution of the problem to find the figure of the earth, (In the philoſophical tranſactions) and the algebraick expreſſion of its nature repreſented by an equation of four dimenſions; hence there is ſome ſmall fault amongſt the *Newtonians* in this, *viz.* to call the real figure of the earth by the name of ſpheroid, when all geomatricians before, by that word, never meant any other ſolid, but that generated by the motion of a conick ellipſis about its axis. The conick ellipſis is only a line of the ſecond order, but that other oval is a line of the fourth order; theſe gentlemen ſhould not deſtroy a proper diſtinction firſt uſed and eſtabliſhed by themſelves.

PROPOSITION VIII.

To enumerate ſome of the aſtronomical operations, which are very eaſy for every body almoſt to apprehend how to perform, in theory and ſpeculation, but in the actual practice are often difficult, and may be liable to much more error than they are aware of.

TAKING Zenith diſtances of the ſun or ſtars, drawing meridian lines upon the plane of the horizon, obſerving meridian altitudes, finding the difference of longitude and other operations by quadrants, and ſextants, &c. are attended with ſeveral difficulties, and oftenofentimes ſubject to error more or leſs, in whatever parts of the earth theſe things are done. But at places ſituated in the high latitudes, *viz.* beyond 65 degrees, ſome of theſe difficulties become the greater, as well as the errors of the performance.

It does not occur to all who talk of theſe things, and underſtand how to perform the operations in general, that at that ſpot of the earth under the north pole, (and the ſouth too paſſing through it) any line upon the plane of the horizon continued both ways ſtraight forwards will be a *meridian line.* So that in this place there will be an infinite number of meridian lines all round the horizon, inſtead of one only, which every other place of the earth has except thoſe two under the north and ſouth poles.---Here the common ways of drawing a meridian line uſed in the leſſer latitudes, by taking the ſun's greateſt or meridian altitude on any given day, will be ineffectual, and ſo will that by the meridian altitudes of the ſtars; for any altitude of the ſun or ſtars is a meridian altitude at this place, and all the altitudes in the ſpace of 12 or 24 hours, may be taken for the ſame, or not much different, and are there no two equal increaſing and decreaſing altitudes in the ſpace of 12 or 24 hours, as there are in the mornings and afternoons the ſame day at the places ſituated in the leſſer latitudes. Moreover, under the pole all the points of the compaſs are confounded, and loſt, except the north and ſouth, there being no eaſt or weſt or any other of the intermediate points between them. Conſequently all difference of longitude between this polar ſpot and any other place of the earth, in the leſſer latitudes does become nothing. That is, this ſpot has no longitude, according to the meaning of the word, and where there is no longitude *there*, there can be no difference of longitude.

Hence it is eaſy to conceive that any place not far from the pole, which has indeed a meridian line, but how liable to error all the operations muſt be by which it can be traced upon the horizon; nor by eclipſes of the moon, or the beſt clocks, can any thing be done exactly towards finding the longitude of any place whoſe latitude is ſo much as 70 degrees, and then the difference of longitude between this place and any other lying more to the ſouth, cannot be found out and depended upon, to any tolerable exactneſs.---Hence the meridian line drawn at *Tornea*, ſaid to be in the latitude of 65 degrees and 50 minutes and 50 ſeconds, by the *French* mathematicians who meaſured a degree at the polar circle, in the year 1737, muſt be liable to ſome error. Alſo the longitude of *Tornea*, is not eaſily to be truly determined, although theſe gentlemen put it at 1^h 23$'$ eaſt of *Paris*. But they themſelves have even ſome doubt about it. They found the latitude of *Tornea* to be as mentioned above, by means of a quadrant of 2 feet radius; and ſet down the ſeconds, as if ſeconds could be actually and truly taken by an inſtrument of ſo ſmall a radius. By means of this Quadrant alſo (ſee page 196,) they have ſet down the longitude of *Tornea*. At page 182 they put down the obliquity of the ecliptick at 23°. 28′. 20″. differing in this from the generality of aſtronomers, who now a days make it to be 23°. 30′. But herein I ſhall think them wrong, till I am convinced by ſome more proper authority, and ſhall take all their concluſions founded upon this not to be ſtrictly true.

Theſe *French* gentlemen, in the preface to their book, tell us, that Meſſ. *Spole* and *Bilberg*, the two mathematicians which *Charles* the XIth. king of *Sweden*, ſent in the year 1695 to *Tornea*, to make ſome aſtronomical obſervations, concluded the elevation of the pole there to be 65°. 43′. *while*, ſay they, *if they had employed the proper data, they ſhould have found it to be but* 65°. 40′. even according to their own obſervations; and *that the town of* Tornea, *is* 11′ *minutes more to the north than theſe two Swediſh mathematicians*

maticians made it to be. Now the proper data above mentioned, according to the *French* gentlemen, ſhould have been the obliquity of the elliptick, the ſun's parallax, and the refraction; to all this I ſhall ſay, the obliquity of the elliptick, viz. 23°. 30′. uſed by the two *Swedes*, ſeems to be more exact than the *French* gentlemens of 23°. 28′.--- That the ſun's parallax of 10 ſeconds, is doubtful, and the refraction at *Tornea* as well as at other places is not conſtant and always the ſame, that the *French* gentlemens quadrant of 2 feet radius was too ſmall to take the zenith diſtance of the pole ſtar exactly, at *Tornea*, that they have ſet down the ſeconds when ſuch a quadrant may not perhaps truly give the minutes in that zenith diſtance; ſo that I am no more certain of the truth of their determination of the latitude of *Tornea*, than I am of that of Mr. *Bilburgh*.

Again, theſe *French* mathematicians will have the refractions at *Tornea* not to be more than they are at *Paris*, altho' Mr. *Bilburgh* concludes the refraction at *Tornea* to be greater than it is at *Paris*. Moreover, they ſeem to leſſen the veracity of the account of the *Dutch* who wintered at *Nova Zembla* in the year 1597, who found the ſun's horizontal refraction to be 4 degrees. But granting the truth of the obſervations of theſe *French* mathematicians, and that the refractions at *Tornea* at the times they made them, were no more than they ſay, yet ſince the refractions are not always the ſame, at the ſame place and in the ſame parallel of latitude, but are greater when the denſity of the air is greater, or when the air is filled with more froſty particles, or other corpuſcles whoſe figures do more refract the rays falling amongſt them, I ſay the refractions at *Tornea* will ſometimes certainly be found to be much greater than theſe *French* gentlemen found them to be, their number of obſervations on this head are too few and ſcanty, to build ſuch a general aſſertion upon, as that refractions at *Tornca* are not greater than they are at *Paris*. Theſe are greater and leſs at *Paris* too, as well as any where elſe. Wherefore I muſt conclude this aſſertion of theirs makes out nothing to the purpoſe; and neither proves Mr. *Bilburgh* to be wrong nor leſſen the truth of the *Dutch* who wintered at *Nova Zembla*, in their accounts of the greatneſs of the refractions at *Nova Zembla*, which theſe *Dutch* make to be in the latitude of 75 degrees and about 25 minutes. The relation of theſe *Dutch* ſailors is both curious and uſeful. In the acta eruditorum for the month of *Feburary*, in the year 1697, at page 82, it is ſaid that the *Dutchmen* who paſſed the winter at *Nova Zembla*, in the year 1592, obſerved the ſun diſappeared from them on the 4th day of *November*, and upon the 24th day of *January*, it began to riſe again, which is ſix days ſooner than it ſhould be by the aſtronomical computation; the ſun's upper limb being then 4 degrees and 30 minutes below the horizon, that is, the horizontal refraction of the ſun was then 4 degrees and 30 minutes by the computation; thus far the acta eruditorum. Now thoſe who are not willing to think the horizontal refraction above related, can be ſo much as 4 degrees 30 minutes at *Nova Zembla*, may if they pleaſe ſuppoſe it to be leſs, and that this diſappearance of the ſun ſooner than it ſhould be by the computation founded upon the ſpherical figure of the earth, was really cauſed by the irregular figure of the earth and its parts at and beyond *Nova Zembla*. Perhaps the land or ice, &c. to the northwards were higher than they were about *Nova Zembla*, and by that means the ſight of the ſun was obſcured by them ſooner than was expected. This might alſo happen, if the figure of the earth was a long ſpheroid, as the *Caſſineans* will have it to be. But the truth of all this, which can only be found out by experience, will never be obtained; for I don't ſuppoſe but that men are now become wiſe enough, not to go again to ſuch diſmal, and dangerous places, &c. as *Nova Zembla*, to make experiments of any kind whatſoever, and hazard their lives, and the loſs of their ſhips, for nothing that can produce a reaſonable and adequate equivalent for ſo doing; ſeveral of our brave and able countrymen, ſuch as the captains *Davis*, *Hudſon*, *James*, and but few years ago captain *Middleton*, ſailed into the cold northern ſeas to diſcover north-weſt paſſages, but found none, becauſe I ſuppoſe there were none, and returned with danger, loſs, and repentance.

PROPOSITION IX.

The diſtances of places upon the earth, ſituated on the ſea ſhores eſpecially, of whatſoever figure and magnitude the earth is, can certainly be beſt and trueſt determined by the inſpection and compariſon of the journals of the ſeamen who have ſo often ſailed to and from thoſe places.

IF for example, the diſtance between *Gibraltar* and the *Lizard* be to be found. Their diſtance can ſcarcely be doubted, ariſing from ſome of the journals of the ſhips that have ſo frequently ſailed between thoſe two places.——For if the leagues of diſtance, as found by the beſt journals of ſeveral ſhips, be but very little different, any one of thoſe diſtances may be taken for the diſtance between *Gibraltar* and the *Lizard* without any great error: but if that diſtance be found, and ſet down to be different by the journals of ſeveral ſhips, many of thoſe ſeveral diſtances ſhould be taken and added together, and the ſum divided by the number of aſſumed diſtances : then the quotient will give a mean diſtance, not very remote from the truth ——For every expert ſailor by long uſe and frequent practice, knows the rate of going of his ſhip per hour or day, with ſuch a wind, and courſe, and ſeldom fails much of his right reckoning and eſtimation of the diſtance between two places, unleſs his voyage be attended with ſtorms and bad weather, and other croſs accidents, that he could not foreſee or be guarded againſt—Therefore by means of ſea journals, enough of which may be eaſily gotten from the captains of ſhips, and other ſea officers, &c. already prepared and fit for uſe; tables of the diſtances of places upon all the points of the compaſs may be ſet down for the future uſe of others, who are to ſail to thoſe places, much more to be relied upon for exactneſs, than the diſtances of places obtained from the latitudes and longitudes of the places by trigonometrical computation founded upon theory and hypotheſis; and the uncertain figure and magnitude of the earth; and by help of ſuch journals, the true figure and magnitude of the earth may be attempted to be found out, if it be nearly a ſphere or ſpheroid, or other ovalar ſolid, of ſome higher regular geometrical order.

PROPOSITION X.

To cenſure and condemn a book with a bold title page, publiſhed about ſix years ago, called, NEW PRINCIPLES OF GEOGRAPHY AND NAVIGATION, *containing the theory of the true figure and dimenſions of the earth, deduced from actual menſuration, and directly charging all the common principles of geography and navigation with falſehood, and all the maps and charts, with* MERCATOR'*s method of ſailing, &c. to be erroneous and uſeleſs.*

ALTHOUGH I have already ſhewn that it has not been proved ſufficiently by experience that the earth's figure is nearly ſpherical; I am far from poſitively aſſerting it not to be ſpherical, or near enough to that figure, as to cauſe very ſenſible errors in geographical concluſions, and I heartily wiſh it may be ſo, for the ſake of navigation and aſtronomy. The ſimplicity of that figure above all others, renders every operation, as well arithmetical as geometrical, the moſt eaſy and natural poſſible, whether they conſiſt in part of theory, or part of practice. We have all been long uſed to, and like ſpherical trigonometry, both for its ſimplicity and the facility of the operations; and ſo we do the ſeveral projections of the circles of the ſphere upon a plane: therefore all innovations and alterations in theſe things ſhould be firſt well weighed, and their truth eſtabliſhed upon firm grounds, moſt convincing experience, and ſound reaſoning, before they ſhould be admitted and brought into practice and juſt eſteem, amongſt wiſe, honeſt and impartial perſons. —— Whoever upon preſumptive and inſufficient experience, endeavours to prove, that the principles of navigation hitherto eſtabliſhed by all men, upon the ſpherical figure of the earth, and introduces a new and difficult doctrine of finding the ſides and angles of ſpheroidical triangles, and delineating their projections upon a plane ſurface, does manifeſtly abuſe uſeful ſcience, impoſe upon mankind, and play the fool with them and himſelf. Who can approve of this new ſpheroidical trigonometry, which at beſt has no other foundation for its exiſtence than the *Newtonians* opinion concerning the flat ſpheroid of the earth.——Geography and navigation are of too much importance to be trifled with; nor are they to be proſtituted by the intereſt, pride or ignorance of any one man and his followers, whatever be his abilities. Both Sir *Iſaac Newton* and Mr. *Huygens* were undoubtedly the greateſt mathematicians in the world: theſe two, as I have already ſaid, were the firſt aſſertors of the flat ſpheroidical figure of the earth.——

But I am of opinion that neither geography nor navigation has received any real benefit or improvement from this imaginary ſpheroidical figure of theirs. Nay, I am inclined to believe this diſcovery has rather done more hurt than good to theſe arts, however, ſome may think otherwiſe. And if they ſuppreſſed their thoughts and publiſhed nothing about it, it might have been better.——*Newton* himſelf honeſtly owns his flat ſpheroid does not ſenſibly affect the operations founded upon the ſpherical figure of the earth.——But there are of late years others who will make us believe, the real figure of the earth has been found to be a much flatter ſpheroid than Sir *Iſaac Newton* will have it to be ; and upon this ſuppoſition have taken the liberty to explode, reject, and vilify the old art of navigation, and alter the operations; give new maps and charts founded upon this fictitious flatter ſpheroidical figure of the earth.——This is all to be ſeen in the book mentioned in the text of our propoſition, wherein is a table of meridional parts calculated for ſuch an imaginary ſpheroid, from the meaſure of a degree at the equator, by *Don George Juan* and *Don Antoine de Ulloa.* In a word, theſe tables, maps and charts, and the ſeveral new ways of operation from them, are nothing at all without ſufficient proof of their truths by experience; the two *Dons* meaſurement, whether true or falſe, at the equator, being far from giving any real foundation for all theſe pretended faults in the ſpherical navigation and geography, which ſailors, as well as others, have all thought to be the beſt and trueſt, and taught them as ſuch to all the youth committed to their care for a long courſe of years. I therefore have more reaſon to condemn and diſprove the aforeſaid treatiſe of new principles of geography and navigation, than that treatiſe has the old ones, hitherto taught and uſed by all men. And accordingly I do moſt heartily deſpiſe and reject it, and hope all the ſailors will do ſo too, at leaſt till it be found to be the trueſt of the two, by the ſailors themſelves, who are beſt able to compare the new and old methods, by means of their journals of voyages, ſo repeatedly made from place to place, in all parts of the world, and to ſee which of the two foundations, the ſpherical or ſpheroidical is the trueſt and moſt exact.

N. B. I ſhall have ſome ſmall opinion of this work, if the author, or any body elſe, will prove to me by ſufficient experience, whether the equator itſelf be a real circle of the earth. I know indeed, that every body calls it a circle, but I am not to take for granted that it is one without good proof. New opinions, ſtarted by great men, whether true or falſe, are too apt to be adopted and catched by the lovers of novelty. There have always been as many blind followers in the ſciences as there are in religion, and politicks in the ſtate. Who are more diſpoſed to have faith and partiality for their guide, than ſound reaſon and unbiaſſed truth; who believe all upon truſt that is ſaid by him whom they think not to be liable to error, in the ſubject he ſo well underſtands, and is ſo great a maſter of, who prefer the laſt writers to thoſe preceding, and uſually their own countrymen to the foreigners who have treated of the ſame ſubject.

PROPOSITION. XI.

To point out ſome more of the uncertainties and ſophiſtical concluſions contained in Mr. MAUPERTUIS's *treatiſe, entitled, The figure of the earth determined from obſervations made by* ORDER *of the* FRENCH KING *at the polar circle.*

1. AT Page 3 of the *Engliſh* tranſlation, it is ſaid, "the advantages ariſing from the diſcove-" ry of the earth's true figure, go beyond

"mere ſpeculation; they are real, and "of very great importance."——If the earth be a ſphere or ſpheroid, flat or long, it is certainly of great uſe to find out which of the three it is, and the lengths of the degrees of the meridians and parallels. But if the figure of the earth be irregular, the diſcovery thereof by experience is of ſome advantage, it is true, but not of ſuch vaſt importance as the author of the preface thinks. He ſuppoſes it to be a ſphere or ſpheroid, flat or long, before he really knows which of the three it is, or whether it be either; and thence concludes, that its meaſurement and dimenſions will be of vaſt importance to navigation, geography, and aſtronomy. I ſhall ſay ſo too, after I am convinced by full experience. But as this conviction is but ſmall and inſufficient, the importance deduced from it cannot be truly ſaid to be very great.

2. At page 4 it is is ſaid, " if the longitudes and latitudes of places were " ever ſo exactly marked down upon " our globes and charts, it would ſignify " little to the finding their true diſtances, " while we were ignorant of the length " of the degrees of the meridian, and of " the parallels to the equator; and if the " diſtances of places are not very well " known, to what dangers muſt the ſhips " be expoſed that are bound for them?"

Now to this I anſwer, that many captains of ſhips ſail from one port to another, without knowing their diſtance. Their method is to keep cloſe along the meridian of the place ſailed from, till they find themſelves in the parrallel of the latitude of the place they want to arrive at. And then they ſail along that parrallel till they are got to the place they want to come to; this is a ſure and ſafe way, although it be round about, and ſomewhat like going over the two ſides of a triangle when one might do. If the figure of the earth be not a ſufficient foundation for obtaining the diſtance of places, and their true courſes, yet the ſailors, by much experience and often going to places, know their diſtance and courſes tolerably exact, without making any great account of the figure of the earth, be it what it will.

3. At page 5, the author ſays, " what " the antients have done and given us " about the meaſure of the earth, are ſo " different and inconſiſtent, and the exact " length of their *ſtadia* and *miles* ſo un- " certain and unknown to us, that what " they have left us upon this ſubject is " very little to be depended upon. That " in latter times, ſurveys were made, free " indeed of the inconveniences laſt men- " tioned, but they were almoſt as inſig- " nificant as thoſe of the ancients.—— Now I think the author here talks a little too much at random, and blames the ancient meaſures of a degree of the earth without juſt cauſe, becauſe they were different, without truely knowing whether thoſe differences aroſe from the errors of their operations, or were

the real differences that muſt ariſe from true operations at ſeveral places and times. The length of a degree deduced from very exact meaſurements made in different parts of the earth, may not be the ſame, if one meaſurement be made upon land or water having a low ſituation, and another upon land or water that is conſiderably higher, and that all lands and waters are not of the ſame heighth, or are equally diſtant from the centre of the earth, is not to be doubted. Every body knows that *Egypt* has a low ſituation, and ſo has *Holland*. Now *Ptolomy* of *Alexandria*, from meaſurements made in *Egypt*, makes a degree to contain 500 ſtadiums, ſo does *Strabo*, and *Poſidonius*, and *Myrinus Tyrius*: there is no doubt with me but this is nearly the length of a degree in *Egypt*, as found from meaſurements made there 17 or 18 hundred years ago; the difficulty conſiſts in knowing what was the length of one of thoſe ſtadiums, or how many *Engliſh* feet one of them did contain. In doctor *Jurin*'s

Jurin's appendix to *Varenius*'s geography, we find that the *Engliſh* foot is to the *Roman* foot, as 100 is to 97; and it is related by *Hero* the mechanick, that 5 *Alexandrian* feet, were equal to 6 *Roman* feet; and according to *Herodotus* and *Suidas*, a ſtadium is 600 *Egyptian* feet. Now granting theſe to be true, there will be 257725 *Engliſh* feet in a degree of *Ptolomy*'s.—Alſo from a meaſurement made in *Holland*, above 100 years ago, Mr. *Snell* found the length of a degree (if the *Engliſh* foot be to the *Rynland* foot as 100 is to 103) to be 353306 *Engliſh* feet: and Mr. *Norwood*, in *England*, in the year 1635, by meaſuring the diſtance from *London* to *York*, found the length of a degree to be 367196 feet. Alſo Mr. *Caſſini*, about 45 years ago, from meaſurements made in *France*, makes the length of a degree to be 343752 *Paris* feet, or 366669 *Engliſh* feet. Now, why may not all theſe different lengths of a degree of the earth be ſufficiently near the truth, and why ſhould one think but ſuch performers as *Ptolomy*, and *Snell*, and *Norwood*, and *Caſſini*, were too able, and circumſpect to be deceived either in their meaſurements, of the diſtances or taking the differences of the latitudes of places, or in any thing elſe that would lead them into much error. I therefore am of opinion that the irregular figure of the earth may be the chief cauſe of thoſe different lengths of a degree, and not any great and erroneous operations committed by any of the meaſurers. *Ptolomy* was a great mathematician, as appears by his book of the great conſtruction. *Snell* was a very good one too. See his *Cyclometricus* and *Eratoſtenhes Batavus*. *Norwood* was a very able, honeſt and exact teacher of practical mathematicks, as appears from a very good book of his called the *Seaman's Practice*; in the preface whereof he ſays of himſelf, *I acknowledge to have had my living and maintenance by the mathematicks, and not by ſpeculation only, but rather by my practice therein; and thereſore have ſpent in ſome principal parts of mathematicks near as much time and pains in experimental practices and concluſions, as in the ſpeculation.* As to Mr. *Caſſini*, few who have ſeen his works can doubt the truth of his concluſion and exactneſs in the length of a degree which he has given us. I don't know but the diſtance of two cities is alterable in a great length of time, by the augmentation or diminution of the heighths of the cities themſelves, and of the lands or waters lying between them. Moreover, there is no body can be certain whether the earth has any one centre of magnitude at all, nor whether the whole, or parts, or arches of ſome terreſtrial meridians in different countries may not have different centres and radius's. All this may happen from the irregular figure of the earth, and from the alteration of the direction and force of gravity, at different places and times. However this be, I cannot be perſwaded, but that the lengths of a degree given by thoſe able men abovementioned, are near the truth; although they be different. As to *Ptolomy*'s, there may be a doubt about the true length of a ſtadium he uſes. But that his concluſion is wrong, I cannot believe.

4. At page 10, it is ſaid, "At laſt the

" king [of *France*] ordered the lengths of " a degree to be meaſured, at the equator and at the polar circle; that the " compariſon of one of theſe degrees " with that in *France* might deſide whether the earth was long or flat; and " that at the ſame time their compariſon " with each other might determine the " earth's figure in the exacteſt manner." This order of the king of *France* was infinitely laudable, and, I have often wiſhed other kings and princes would take pattern from him, and have ſent out fit perſons, with proper inſtruments, to ſeveral parts of the earth, to purſue the ſame undertaking and laudable deſign. Then indeed we might have been furniſhed with a numerous and ſufficient data, *viz.* the lengths of ſo many degrees at proper places as might enable geometricians to determine the earth's figure, if it were not too irregular.---But the order above related, is by much too inſufficient to anſwer the intended purpoſe; for one meaſurement of the length of a degree at the polar circle, and another at *Paris*, with a third at the equator, will not determine

mine whether the whole earth be longer at the poles than at the equator. Nor if it were, can any juſt determination of the earth's figure be obtained from any compariſon of the lengths of thoſe three degrees at the polar circle, in *France*, and at the equator; and much leſs of two of them, *viz.* at the polar circle and in *France*, as Mr. *Maupertuis* (and others) think they can do. See his problem already mention'd by me at prop. 7. The fallacy lies here; they take for granted the figure of the earth to be ſpheroidical whithout firſt being aſſured that it is really ſuch.

5. And ſo at page 11. what the author ſays about the flat ſpheroid of Sir *Iſaac Newton*, and the long one of Mr. *Caſſini*, of ſailors fancying they are ſailing upon one of theſe ſpheroids, when they are really ſailing upon the other, is but miſapplied caution and mere fiction. Good ſailors don't trouble themſelves much about ſpheroids; ſome don't know them at all; nor did ever any man amongſt them think the figure of

the earth was a ſpheroid before Meſſ. *Huygens*, *Newton* and *Caſſini* put it into their heads. All theſe ſpeculative mathematicians have been too great dealers in, and retailers of uncertainties amongſt mankind, many of which have no other real exiſtence than in their authors own imaginations. But here I except Mr. *Caſſini*.

6. Mr. *Maupertuis* at page 62, 63 ſays, “ the arch of the meridian of 57′. 27″. “ he and his aſſociates went to meaſure, “ was of a juſt length to give the opera- “ tions the greateſt degree of certainty, “ and that an arch much longer or much “ ſhorter, would not ſo well have ſerved “ his purpoſe.” Now, I believe every body will eaſily grant that an arch which was leſs than 57′. 27″. was too little. But that a bigger arch would not be better, I neither can or will ever believe. The author here is miſlead by his own opinions, and might almoſt as well have ſaid the whole circumference of a circle will come out more exact by

having given the length of a ſmall arch of it, and the angle at the centre ſubtending it, than if that given arch and angle had been bigger.

7. The laſt chapter of Mr. *Maupertuis*'s book of the meaſure of a degree at the polar circle, contains a problem ſhewing *how to find the angle which the direction of the actual gravitation makes with the direction of the primitive gravity, or that point of the earth's axis to which gravity tends, by having given the figure of the earth, as alſo the proportion of gravitation at the equator, to that of any given latitude.*

But this problem is merely ſpeculative; it is only an exerciſe of the mind of a curious mathematician, whoſe ambition is to excel his brethren in fictitious computation, and ſhew the world, by his reſolution of that problem, how to find an angle, that no where really exiſts, unleſs in his own imagination. This has a tendency to perplex maſons and bricklayers, and make them build their walls not upright as uſual, for fear they ſhould fall down.——In ſhort, the problem is uſeleſs, becauſe no man can tell by experience whether it be true or falſe.

PROPOSITION XIII.

Any manner of finding the longitude is attended with difficulties and uncertainties, and ſhould not be approved of, and uſed, before it be tryed by ſufficient experience, and found to be good.

1. SINCE the longitude of moſt places at land, are found out by the eclipſes of the moon, or thoſe of Jupiter's

Satellites, it ſhould be firſt conſidered and tryed by many experiments, whether the ſhadow of the earth moves during an eclipſe of the moon, equably, or paſſes over equal ſpaces in equal times. If it does not do ſo, the longitude of places determined by this method will be erroneous, and theſe errors will affect the finding of the longitude at ſea or land by a clock or time-piece, or any other way.

2. We are not certain, as I have ſaid already, that the parallels of latitude are plain figures, much leſs circles; nor whether all the meridians be equal circles or ellipſes, or even plain figures; nor whether the earth has any centre of magnitude at all; nor whether the earth's axis be a ſtrait line or not. Theſe doubts ought to be removed by ſufficient experience; the truth of aſtronomical and geographical poſitions is rather to be obtained from much experience and careful obſervations, than by hypothetical aſſertions, wherewith aſtronomy

to me ſeems to be too much clogged and corrupted.

3. It does not appear to me that men can contrive any time-piece, clock or watch, that will give the true hour in all places of the earth, and at all times exactly correſponding with that of the ſun, at thoſe places and times.

4. Allowing the ingenious Mr. *Harriſon*'s time-piece to be the beſt that was ever made to meaſure time at ſea, or go with the ſun, yet I think it can do this laſt but in one place or latitude, becauſe it has been found by experience, that at places in different latitudes the pendulum time-pieces and watches do not all go alike, and meaſure the ſame hour with the ſun at thoſe places, and the law of this difference between the going of the clock, watch or time-piece and the ſun, at different places and latitudes being not known, muſt neceſſarily cauſe uncertainty and error in the de-

termination of the longitude by any clock, watch, or time-piece whatſoever.

Beſides, in the ſame or at different places of the earth, the going of clocks and watches is conſiderably affected by the weather. Some gowing faſter or ſlower, more at one time than another, greater or leſs heat, or moiſture of the air, &c.

Even granting Mr. *Harriſon*'s or ſome other time-piece will always go at ſea and every where keep time with the ſun, the longitude cannot then be found by it, unleſs the moment of the ſun's meridian altitude be found within certain limits of exactneſs not eaſily obtained, from obſervations made with quadrants, &c. at ſea. And theſe are liable to errors more or leſs for ſeveral cauſes.

5. For all theſe reaſons and others, whoſoever thinks, and ſays, he has found out the longitude by a clock, watch, or any other uſeful and eaſy way practica-

ble to all thoſe who have a direction of a voyage, muſt convince us and himſelf by experience that he has really done it.— A few experiments ſufficiently near the truth will not be enough ; there ſhould be at leaſt one hundred made at different times, places and parts of the ſeas.

6. I ſhould think it would be beſt by this time-keeping inſtrument, to find the difference of longitude of ſome known cities and places at land, before the ſea longitude be attempted.

If the buſineſs ſucceeds here, that will be an encouragement to proſecute the thing at ſea. If not, the trouble of going to ſea with the clock or watch, and the expence too will be ſpared.

But if in one hundred voyages with ſuch a time-piece to different parts made in all varieties of weather, times and places, thoſe voyages have been made ſooner, and the ſhips tract and place in the ocean better known than by the common methods uſed before, its excellence will then be diſcovered and approved of, and the author rewarded for ſuch a diſcovery.

PROPOSITION XIV.

Some aſtronomers tell us, the naked eye cannot perceive in the hemiſphere more than 1000 *fixed ſtars in the cleareſt ſtar-light night, when the moon does not ſhine. And that the ſharpeſt ſight will not diſcover one ſtar more even of the leſſer magnitude than is marked down on the celeſtial globes, ſuch as thoſe of* BLAVE, *&c.*

NOW this aſſertion I take to be a moſt evident falſehood, for inſtead of ſeeing but 1000 ſtars on ſome clear nights, I am certain I have ſeen ſome thouſands of them. Let any one conceive the ſtarry hemiſphere to be divided into four equal parts by two verticle circles, or partitions interſecting the horizon.——If a perſon does not perceive and count more than 250 ſtars in each of thoſe parts, I never ſaw a ſtar in my life, nor could count 250 of them.——And if there were ten vertical partitions, there would be more than 100 ſtars to be ſeen in each of them. Therefore, ſince this aſſertion of the ſmall number of the fixed ſtars is ſo contrary to the experience and reckoning of every body elſe, who looks at them, it is a wonder to me how theſe aſtronomers ſhould be ſo much miſtaken as to affirm the naked eye ſees ſo few as 1000 at once, when every body elſe think they ſee ſeveral thouſands.——It is true, indeed, all the aſtronomers do not maintain this glaring and evident falſhood. Some writers of the numbers and orderly placing of the fixed ſtars, I believe have been miſtaken from the words of others about their number. The ſew good aſtronomers, as I take it, ſay all the ſtars that have been obſerved and put into a catalogue, with their ſeveral longitudes and latitudes, have been 2000; but don't ſay or mean the number of all the ſtars they have ſeen with the naked eye are not more than 2000; and thence ſome careleſs aſtronomers miſtaking others in this matter, have made the number of the ſtars to be too ſmall. But the following reaſoning given by them to ſupport this aſſertion, I take to be fallacious and nothing at all, *viz. they ſay the ſtars at firſt appear to be innumerable indeed to any one who looks at them. But this they would have to proceed from the vehement ſhining or twinkling of the ſtars while the eye looks at them confuſedly and diſorderly.* I never ſaw any ſuch vehement ſhining or twinkling of the ſtars, that could hinder me from counting more than 1000 ſtars on a clear (froſty) night.

In Doctor *Gregories* aſtronomy at prop. 29, book the 2d, there is an account of the catalogue and number, &c. of the fixed ſtars, ſufficiently circumſtantial, and in doctor *Keil*'s 6th aſtronomical lecture, there is alſo a ſhort account of their magnitude, orders, catalogues and changes—full of conjecture and uncertainty. Moreover, in Mr. *Flamſtead*'s *Hiſtoria Celeſtis*, is to be ſeen a catalogue of 3000 fixed ſtars, many of which cannot be ſeen by the naked eye without a teleſcope. But to ſpeak the truth, this is but dry knowledge, and the teleſcopick ſtars of his are of but very little real uſe.

PRO-

PROPOSITION XV.

The Aſtronomers do not agree about the Equation of time and the inequality of the length of the natural days at different times of the year. Nor is the Truth of the tables of the equation of time, abſolutely to be relied upon, becauſe they are all founded and computed from ſuppoſitions that are not certain, and diſagree too.

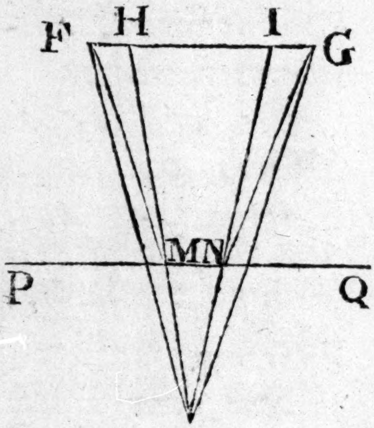
MOST of the aſtronomers will have the mean or equable time to be founded upon a two-fold cauſe, *viz.* the difference between the true motion of the ſun and its right aſcenſion turned into time, and upon the unequal motion of the ſun in the eclíptick, having recourſe to a fictitious ſun to help them to explain the thing, and make a table of the equation of time or the natural days. But theſe tables, as they ſay, will not laſt long; for in a few years they will become uſeleſs and new ones muſt be made. *Ptolomy*, at chap. x. of his third book of his *great Conſtruction*, expreſly treats of the equation of time being thus cauſed. See alſo Dr. *Gregory*'s aſtronomy book, 3 prop. 17, and Dr. *Keil*'s aſtronomical lectures. — But *Vendeline* and others will have all theſe tables to be falſe and uſeleſs, and makes no difference between the apparent and mean time. — On the contrary, *Kepler* ſet thoſe aſide, and had a ſuſpicion of a third cauſe of the inequality of the natural days, occaſioned by the inequality of the motion of the earth about its axis, which is not quite equable, but grows greater or leſs according to its leſſer or greater diſtance from the ſun, the fountain of its motion. And this part of its motion he called *phyſical*, and the other two *demonſtrative*.—*Tycho Brahe* too had a ſuſpicion of an acceleration and leſſening of the firſt motion, and from thence the other part of the equation he left to be conſidered; and has only given that ariſing from the obliquity of the ecliptic, becauſe it was ſufficient to ſubſtract it from the calculation of the eclipſes obſerved by him.

Hence it is evident to me, there is no making true tables of the equation of natural days, but by long, manifold and exact experiments, with good pendulum clocks, and exact meridional planes, ſet up at different places of the earth, as well under the ſame meridian, as under different meridians, thereby to diſcover how much ſooner or later than 24 hours the ſun comes to the meridian, one day than he does on another; and doing this for ſome years, muſt certainly be the only means to obtain tables of this kind free from any one or more uncertain ſuppoſitions, and ſufficiently true.—This is the way to know the quantity of the inequality of the natural days, or whether there be any conſiderable difference in their lengths, worth while to take notice of, and effect the truth of aſtronomical calculations in any great degree. — Nothing is more common than the tables of the equation of time; you will frequently ſee one of them at the bottom of a watch caſe, or marked upon an horizontal dial. But of whatever uſe they may be in aſtronomy, yet in common affairs of life, they do and always did appear to me to be trifling and uſeleſs.

PROPOSITION XV.

Since moſt of the aſtronomers and opticians, will have it that the Æther above the atmoſphere, and this laſt, are as two contiguous mediums of different denſities, and the æther the rarer of the two; and accordingly the atmoſphere attracts the rays of light moſt coming from the celeſtial bodies, and the more ſtill according to its greater denſity. It will follow from the laws of opticks, that the apparent diameters of the *moon and planets, viewed by an eye in the air, will be the leſs the more denſe the air is; which is contrary to real experience.*

FOR the optical writers, ſee Dr. *Barrow's* 15th optical lecture, ſay an object in the water appears to be bigger to an eye in the air viewing it, than if that object was in the air. But as this is not true from the optical reaſoning, when that object in the water is ſmall or very remote, ſo as the rays coming from its extremes fall upon the eye, nearly parallel, it ſhould be tryed how the thing is in water when the object is either ſmall, or if big it be very deeply immerged, ſo as that its rays coming from its two extreme points fall upon the eye nearly parallel. On the contrary, when the object F G in the air, and the eye be ſuppoſed to be at O in the water, whoſe ſurface is P Q, either plain or ſpherically convex next to the object. If OF, O G be the rays coming from F and G



its extremes to the eye before there was any water, the apparent magnitude of the object F G is uſually repreſented by the angle F O G at the eye. But when there is water, and the eye is within it, for a ray coming from F to fall upon the eye at O, its part F M muſt fall at M on the ſurface of the water towards Q, that ſo the refracted part M O may fall upon the eye at O. In like manner, the ray coming from G muſt fall at ſome point N of the ſurface of the water towards P, that ſo its refracted part N O may fall upon the eye. Wherefore, if O M, O N be joined and continued out to cut the object in the points, H and I, the angle H O I will be the apparent magnitude of the object F G. But the angle H O I is leſs than the angle F O G, wherefore the eye at O in the water ſees the object F G in the air leſs than it would appear to be, if the eye was in the air, and there had been no water to refract the rays; and the greater the refraction of the medium wherein the eye is placed is, or its denſity the leſs ſtill will be the apparent magnitude of the ſame object in the ſame given rarer medium. Hence the eye in the air or atmoſphere, which by all is taken as a medium refracting light to the heighth of 30, 40 or 50 miles, and the celeſtial medium wherein are the ſun, moon, and planets, being not endued with any refractive power at all, will perceive the moon's and the planets apparent diameters to be the leſs when the denſity of the air wherein the eye is placed, is the greater. For the moon's magnitude is too big, and her diſtance from the earth too ſmall to cauſe the rays iſſuing from the extremes of her apparent diameter to fall parallel upon the eye of any man placed in our atmoſphere. Now this is partly the optical reaſoning, according to the principles and theory of that art. But this ſeems to contradict experience, and produce difficulties that I don't know how to account for— but of this I am certain, when the mercury in the barometer is high, as it commonly is in very dry, hard, froſty weather, and in very hot dry weather, in the months of *Auguſt* and *September*, the ſun's and moon's horizontal diameters do appear bigger than at other times, when the weather is either more moderate and inclinable to be moiſt and it rains, and the mercury is ſunk lower. Hence, when the atmoſphere is moſt denſe, the apparent diameters really appear to be greateſt : conſequently the refractive power of the atmoſphere muſt be leſs at or near the horizon, than it is at other times, when the horizontal diameters appear to be leſs. But I leave this to others who know better how to account for theſe things.

Note, If the ſun's rays, when in the zenith of a place, fall parallel upon the ſpherical ſurface of the atmoſphere, as is generally ſuppoſed; its apparent diameter then to an eye in the atmoſphere, will be the leſs (according to the rules of optics) when the atmoſphere is the denſer. But this is upon the ſuppoſition that the upper ſurface of the atmoſphere is ſpherical, and always of the ſame heighth. But theſe are things that we ſhall never know their truth by experience.

PROPOSITION XVI.

The truth of the aſtronomers aſſertions about the magnitudes of the apparent diameters of the ſun, moon, and planets, is not to be entirely relied upon, and free from error.

IN Sir *Iſaac Newton*'s *principia*, page 371, the ſun's mean apparent ſemidiameter is put down at 16′ 6″ and from thence his diameter is 32′ 12″.——In Dr. *Gregory*'s aſtronomy, page 336, the ſun's mean apparent diameter is put at 32′ 15″. In Dr. *Keil*'s aſtronomical lectures, page 86, it is ſaid the ſun's greateſt apparent diameter is in winter 32′ 47″ his leaſt 31′ 40″ in the ſummer. Dr. *Gregory* at page 338 of his aſtronomy, ſays the difference between the ſun's horizontal and vertical diameters as they appear, is not ſenſible — the *French* mathematicians, who meaſured a degree of the meridian at *Tornea*, in the year 1738, will have the ſun's apparent ſemidiameter to be 16′ 19″ [See page 183 of the *Engliſh* tranſlation] whence his apparent diameter will be 32′ 38″ —Now amongſt theſe authors there is a difference of ſeveral ſeconds in the magnitude of the ſun's apparent diameter, and no man can tell which is the trueſt of them, or indeed whether any of them be true—the ſun's apparent diameter is variable,—on the ſame day at one place it is often greater in the morning and evening than it is at noon.—The ſun's meridional apparent diameter is the leaſt at that place of two propoſed ones, whoſe latitude is the leaſt ; and leaſt of all, when it is obſerved at noon in the zenith of a place.—the different denſity of the atmoſphere varies the ſun's apparent diameter at the ſame place, ſeen at the ſame hour, on different days of the year, and when he has the ſame altitude; the denſer atmoſphere, according to the rules of optics, ſhould make its apparent magnitude, for inſtance, at noon, greater than when the atmoſphere is rarer at that time. — Any horizontal ſpherical body at a diſtance ſeen by an eye appears to be much bigger than if it were elevated higher up in the air, altho' it be at the ſame diſtance from the eye in both caſes, or even more remote in the laſt caſe : So that the apparent diameter of the braſs ball on the top of the cupulo of *St. Paul's* church in *London*, when viewed by an eye on the upper end of *Ludgate Street*, will appear to be muchleſs than if it were ſeen at the ſame diſtance on the ground ; and the chief cauſe of this ſeems to be the light which comes from the ball to the eye, in its elevated ſituation, being greater than the light coming from that ſame ball when on the ground, which cauſes the pupil of the eye to be more contracted in the former caſe, than it is in the latter, whereby the angle at the eye becomes the ſmaller, and the anſwerable apparent diameter to it, the leſs. The ſun's horizontal apparent diameter moſt certainly appears at ſome times greater than it does at noon, at the ſame place of the earth, and on the ſame day.——Again, the ſun's meridian altitude at *London*, which is in the latitude of 51 degrees and 32 minutes, being on the ſhorteſt day in winter not above 15 degrees, his apparent diameter taken at this time, when the atmoſphere is generally leſs pure, and the light of the ſun more obſcure, muſt needs be greater than his apparent diameter taken at noon on the longeſt day, when his altitude at *London* is 62 degrees and ſome minutes, and the atmoſphere not ſo denſe as it is in the winter, and his light is then much more glaring and vivid. It has been found by experience that the ſun's refraction (according to *Tycho Brahe*'s table of refraction, in *Varenius*'s geogr. page 240) at 15 degrees of altitude is 7 min. If this be true, the ſun's meridian apparent diameter on the ſhorteſt day of the winter at *London* will be 14″ more than its apparent diameter at noon on the longeſt day, when his altitude is 62 degrees; this being cauſed alone by the refraction of the atmoſphere: and ſince the refraction of the atmoſphere according to *Tycho* and others ceaſe, at the altitude of 38 degrees, the ſun's apparent diameter at noon, when his altitude is 62 degrees, will not be effected by the refraction of the atmoſphere. And ſo whatever this apparent diameter then is, the winters one will be 14″ more.——I don't know whether any of the aſtronomers have taken notice of all this; but I am certain, the obſervations of the ſun's apparent diameters with proper inſtruments, have been made at far too few places of the earth, to obtain certainty in the truth of what has been ſaid about it, by the aſtronomers. Moſt of the obſervations of the ſun's apparent diameter having been made within the latitudes of 55 and 41 degrees north; excepting at a few other places.—All this uncertainty about the truth of the ſun's apparent diameters extends to thoſe of the reſt of the planets, where by reaſon of the ſmallneſs of theſe laſt, that uncertainty becomes greater, and the concluſions from them are more void of the truth.

PROPOSITION XVII.

The Poſition laid down by Sir ISAAC NEWTON *and the reſt of his followers, that the diſtances of remote ſpherical objects are reciprocally proportional to their apparent diameters is not certain nor always true.*

THE truth of my aſſertion here, at leaſt as to this *Newtonian* uncertainty, muſt eaſily be granted by any one who will but conſider what has been ſaid in our laſt propoſition; to which I will add, what Mr. *Wolfe* ſays, (at the ſcholium of the 28th *theorem of his elements of aſtronomy*, page 503, vol. 2d) *viz.* Mr. *Huygens* "has obſerved that "fires and flames may be ſeen at ſuch "diſtances whereat other bodies con-"tained under ſmall angles vaniſh, and "cannot be ſeen at all. Therefore, ſays "Mr. *Wolfe*, the optical theorem of "the apparent diameters of objects "being reciprocally proportional to "their diſtances from the eye does not "hold good, but as long as the ratio of "the diameter of the object to its diſ-"ance is not too great." Now ſince there is ſo much uncertainty in this theorem, it is evident all the aſtronomical theorems and concluſions depending upon, and deduced from it, muſt be uncertain too. Hence the mean diſtances of the planets from the ſun and their true diameters, magnitudes and denſities, compared with that of the ſun, as given by the aſtronomers, have no certainty in them. And accordingly tho' all the modern aſtronomers, except *Kepler*, will have it, that Jupiter is bigger than Saturn, and Venus than Mars; yet this is uncertain. — *Kepler* ſays, the more diſtant the planets are from the ſun, the greater they be. But his demonſtration is merely hypothetical, altho' perhaps it may be true. The *Newtonians* deny this. But their argument has not real experience for its foundation--There is a great difference amongſt aſtronomers about the figures of the orbits of the planets. The antients, with *Ptolomey*, make the orbits of the planets to be circles, and their motions in them to be equable.——*Kepler* will have the orbits of the earth and planets to be ellipſes, in one of whoſe foci the ſun is placed, about which they all move, in ſuch a manner that the elliptick ſector made by a right line drawn from the centre of the ſun to that of the planet, and the part of the greater axis of the ellipſis, is always proportional to the time, wherein ſuch a ſector is deſcribed. Moreover, the *Keplerian* theorem, that the ſquares of the periodick times of all the planets about the ſun are proportional to the cubes of their mean diſtances from the ſun, appears to me to be only hypothetical and uncertain. The *Newtonians* themſelves find fault with *Kepler*'s demonſtration of it. *Kepler* pretends to have found out the truth of the theorem by obſervations. The *Newtonians* take this for granted, and indeed do fairly prove it, upon the ſuppoſition that the orbits of the planets are deſcribed by a projectile motion in a right line, and by a centripital force, reciprocally proportional to the ſquare of the diſtance of the planet from the ſun placed in that focus. Mr. *Caſſini* will have the figures of the orbits of the planets to be ovals, of ſuch a nature that the rectangle under any two right lines drawn from two fixed points in the axis to any point of the curve will be of a conſtant magnitude— this is hypotheſis without proof. —*Ricciolus* obliquely inſinuates that *Kepler* at laſt thought the orbits of the planets were of an egg-like figure; and *George Purbachius* in his theory of the planets, will have the moon's orbit to be of ſuch a figure. This may be true, but if the nature of ſuch an egg-like oval be not known, ſo as to become

come uſeful in the calculation of the places and motions of the planets, it is of no great moment. What if I ſhould put in my mite, and add, that the orbits of the planets are not really ovals or lines of any geometrical ſimple order, neither ellipſes or lines of the 3d or 4th order, but that ſome of them approach ſo near to a circle or ellipſe, or ſome other higher order of curve lines, as that the computation made from theſe ſingle ovals of the motions and places of the planets, will become eaſy and ſhort, and be as near the truth, and anſwer to experience, as well as if the planets had really moved in them, when they do not.

Now ſince there is ſo much uncertainty in theſe things, all that has been ſaid about the ſun's diſtance, magnitude and denſity by the *Newtonians*, or indeed any body elſe, cannot be taken for undoubted truth.——The excentricities of the planets are but hypothetical and ſome of them cannot be true ; becauſe, different aſtronomers put them differently in the orbit of the ſame planet. In ſhort, the phyſical part of aſtronomy, containing the cauſes of the motions of the ſun, moon and ſtars, &c. are ſo various and un-

uncertain, let who will be the authors, either ancients or moderns, that every man of a good judgment and unbiaſſed mind, who is arrived to a due courſe of years in age, cannot place much belief upon many things that are to be found in this part of aſtronomy; herein faith has much more to do than ſound reaſoning, and convincing proof.—I have ſaid already in the preface, fifty years ago I read and underſtood many of the propoſitions of Sir *Iſaac Newton*'s *principia*, and Dr. *Gregory*'s *Aſtronomy*, about the cauſes of the motions, &c. of the planets, and believed every thing to be true in their books. But now my opinion is much altered about the veracity of very many of them: others of his propoſitions are pleaſant and diverting to a curious mind, but are rather amuſements and uſeleſs novelties than any thing elſe: the demonſtrations are quite out of the reach of the generality of mankind; none but good geometricians can comprehend them, and theſe not without trouble and great application of mind ſometimes. The truth of the mathematical part is certain, tho' difficult enough very often to get at. But the poſtulatums of the univerſal gravity and mutual attraction of the celeſtial bodies, are fictitious, and perhaps not true.

PROPOSITION XVIII.

The 4th Theorem of Sir ISAAC NEWTON'S *3d Book of* Mathematical Principles of Natural Philoſophy, *viz. that the Moon is retained in her Orbit, by the very ſame Force, which we call Gravity, is not ſufficiently demonſtrated by him, nor any of his Followers.*

SIR ISAAC NEWTON'S demonſtration of his 4th propoſition, abovementioned, depends chiefly upon two things, which he takes for granted. The firſt is, that the moon's mean diſtance from the earth, when ſhe is in a line joining the centres of the ſun and earth, is 60 of the earth's ſemi-diameters. Now as *Newton* and his followers will have the figure of the earth to be ſpheroidical, and accordingly the ſemi-diameters thereof are not all equal between themſelves as in a ſphere they are; the accuracy of his demonſtration muſt be leſſened upon this account, in ſome degree at leaſt. But this not all; I venture to ſay, *Newton* himſelf never was ſure by his own experience, whether the moon's mean diſtance from the earth, when ſhe was in her *Syzygies*, was always fixed and invariable; and that it was 60 of the earth's ſemi-diameters. He only takes this upon truſt, from others, and ſays, *moſt aſtronomers will have it to be* 59, Vendeline 60, Copernicus 60[1], *and* Tycho Brahe *will have it to be no more than* 56½ *of the earth's ſemi-diameters*.—But as 60 ſemi-diameters is thought by *Newton* to be the neareſt the truth, and beſt ſuiting his demonſtration of the 4th theorem above; *Newton*, ſomewhat unfairly, as I think, falls foul on this determination of *Tycho*'s, and he would make us believe that it is not true. But what *Newton* ſays is not enough to overthrow its veracity, at leaſt with me, when I conſider *Tycho Brahe* as a very able practical aſtronomer, who has left behind him a vaſt number of obſervations of the moon, and other of the celeſtial bodies, which all the good aſtronomers ſince his time have uſed upon many occaſions, and taken to be true. *Tycho*'s authority muſt therefore have a ſhare in leſſening the veracity of *Newton*'s cenſure abovementioned.—Be this as it will, the ſecond thing upon which *Newton*'s demonſtration is founded, is that heavy bodies fall perpendicularly downwards by the force of gravity, near the ſurface of the earth $15\frac{1}{12}$ *Paris* feet, in one ſecond of time; the truth of which I will readily grant, that heavy bodies at *Paris* were found to do ſo by Mr. *Huygens*, *Mairan* and others, from experiments made with pendulums at *Paris*. But then it did not perhaps occur to *Newton* while he was framing the demonſtration of his 4th propoſition, abovementioned, that falling bodies at places of leſſer latitudes than that of *Paris*, will fall more than $15\frac{1}{12}$ feet in one ſecond of time. For ſince *Newton* himſelf, at page 388 of his Principia, (the ſecond edition) will have it, that the length of a pendulum vibrating ſeconds, at places near the equator, is two lines ſhorter than a pendulum vibrating ſeconds at *Paris*; and becauſe the length of ſuch a pendulum at *Paris* is 3 feet and $8\frac{1}{2}$ lines of *French* meaſure. A pendulum at the equator muſt be nearly 3 feet, and 6 lines long to vibrate one ſecond of time.——If this be true, it will follow from the computation made from the 25th propoſition of Mr. *Huygens*'s book of the pendulum clock, that falling bodies under the equator will deſcend by the force of gravity more than $15\frac{1}{12}$ *French* feet in one ſecond of time, as they do at *Paris*. Hence *Newton*'s demonſtration is defective, in part and not ſufficiently conducive to the proof of the real truth of the 4th theorem of the third book.——The moon's mean diſtance from the earth, when ſhe is at the full, and upon the meridian of a place under the equinoctial, and the ſpace deſcribed by the fall of a heavy body in one ſecond of time there, ſhould be found by experience; and from theſe it would be more proper to try to build a demonſtration of the 4th theorem, and diſcover its truth or not. NOTE, It is eaſy enough for two perſons in concert, one in a place in ſome northern latitude, and another in the meridian of that place under the equator, to find the moon's diſtance from the earth, when at full, and on the equinoctial; or the moon's diſtance from the earth, when ſhe is at the full or changing, and upon the meridian of two places, may be found, by two obſervations made at thoſe two given places by two different perſons of the moon's altitude when ſhe is on the meridian; for by having given the two altitudes obſerved, and the latitudes of the two places of obſervation, as alſo the ſemi-diameter of the earth, the moon's diſtance from the earth may be eaſily found, without any conſideration of the moon's horizontal parallax. And accordingly it may be tried by experience at what diſtance from the earth ſome full or new moon is, when on the meridian of two given places, and what alteration there will be in that diſtance at any other future full or new moon.——This is the way to know the full or new moon's mean diſtance from the earth; and whether *Newton*'s or *Tycho*'s diſtances above be either of them true. And if both the places of the obſervations were in the ſame latitude, the one north, and the other ſouth, each more than 30 degrees, the obſervers at thoſe two places may find how many feet a heavy body will fall down in one ſecond of time. Then, if inſtead of NEWTON's mean diſtance of 60, and the perpendicular fall of $15\frac{1}{12}$ *Paris* feet in one ſecond, be uſed the two other numbers found out by the obſervers, *viz.* the moon's mean diſtance and the *Paris* feet of a heavy body's fall in a ſecond of time, the truth of *Newton*'s 4th theorem will ſufficiently appear, from his way of demonſtration. If not, his theorem is not demonſtrated, and very likely not true.

Note, The diſtance of *Mars* from the earth may be obtained by two obſervers, at two diſtant places of the earth, both under the ſame meridian, made by two perſons, the one being at one place, and the other at the other place.—If the diſtance of *Mars* be not too great, and the two ſtations of the obſervers be as remote as conveniently can be from one another, the one having north, and the other ſouth latitude.—And at preſent I don't ſee why *Mercury*'s and *Venus*'s diſtances may not be obtained this way. It will alſo then appear by experience, whether the *Newtonian* ſuppoſition, that the force of gravity, as high as to the moon, be reciprocally proportional to the moon's diſtance from the earth.

FINIS.

I hope the Reader will excuſe the following ERRATA.

PAGE 5, line 25 wants a full ſtop. P. 17, r. ſphere. P. 35, r. falacious. P. 41, l. 4, r. $69\frac{4}{11}$. P. 73, l. 6. for are there r. there are. P. 76, l. 4, r. ecliptick. P. 113, r. Keplar. P. 118, l. 11. for than, r. then. P. 125, l. 11, for diſtance, r. diſtant, and for as, r. a.